The SCIENTIFIC MONTHLY

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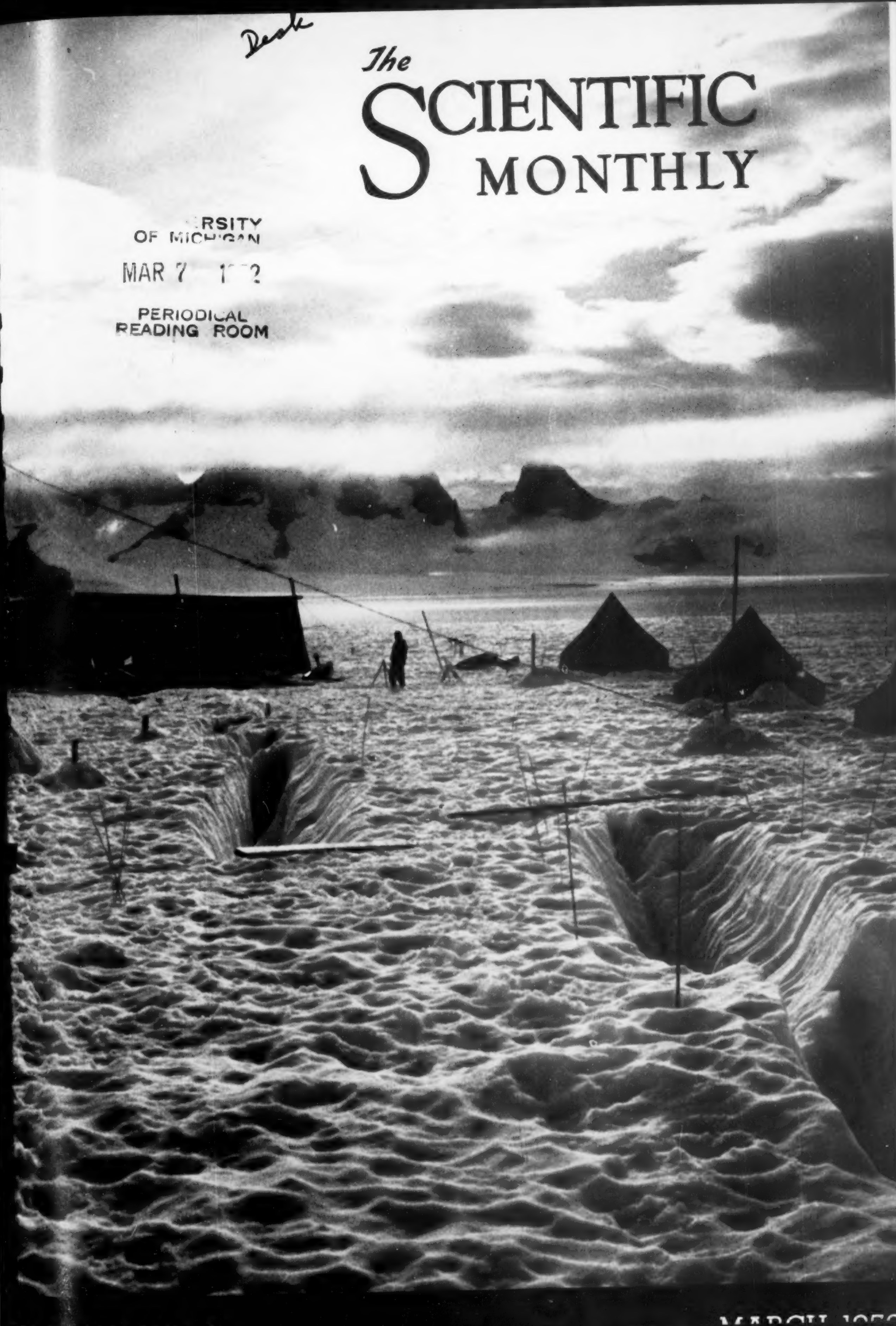

VOL. LXXIV, NO. 3 MARCH 1952

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THE SCIENTIFIC MONTHLY

VOL. LXXIV MARCH 1952 NO. 3

Contents

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Science and Technology

(From the Month's News Releases)

Progress Report

After prolonged research, Stanford University scientists have developed a compound X-ray microscope, using four mirrors instead of two and giving a magnification of 150 diameters. Although electron microscopes, with a maximum resolving power of about a ten millionth of an inch, can be used to examine extremely small objects, they cannot be used to examine living processes, for they operate in a vacuum. The X-ray microscope operates in helium, allowing the use of soft X-rays for the examination of small biological specimens. Research under the direction of Paul H. Kirkpatrick, Stanford physics professor who invented the instrument, will be continued, to develop more sensitivity.

Markers

Plastic-covered labels that include 96 different price units printed on disks die-cut into laminated sheets may be punched out and cemented to the top of price-stamping dowels with a special adhesive. Another marking device is a throw-away bottle with a felt tip that permits fine, medium, and bold marking. When the ink supply is exhausted, the nib section may be attached to a fresh refill. Spillproof, the ink marks with equal ease on porous and nonporous surfaces, is instant-drying, water-, fade-, and launderproof, and is made in nine colors, including black and white.

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A revolving brush, with an attachment holding special detergent tablets, fastens to a garden hose for fast, easy car or window washing. It may also be used to wash boats, screens, walls, porches, and farm equipment. A stationary bumper brush prevents splashing and cleans corners. The brush may be used with either high or low water pressure.

Load Distribution Indicator

By means of an ingenious instrument, the Lodicator, invented by the Swedish engineer Lennart Swenson, it is possible to calculate in minutes the correct trimming and distribution of tanker cargo to avoid longitudinal stress. The device, which has been patented in most seafaring countries, also facilitates unloading, as it ensures correct redistribution of the oil after it has been discharged in different ports.

They Last

Dynel, a completely synthetic product of modern textile research, has been used to design workshirts, trousers, and coveralls that are said to resist acids, caustics, wear, moths, mildew, shrinkage, snagging, and tearing, thus reducing the high cost of replacement in certain industrial operations.

Rare Photographs

A high-speed retinal camera designed by Bausch & Lomb was used at the Pan-American Congress of Ophthalmology, which met in Mexico City early in January, to photograph in color for the first time such unusual diseases as dracontiasis and cysticercus. Either color or black-and-white photographs of the sensitive blood vessels and arteries within the eye may be made with the camera, which was developed for the U. S. Public Health Service. It is used in the diagnosis and treatment of high blood pressure, arteriosclerosis, diabetes, and other vascular diseases. Resulting photographs may be enlarged many times, projected onto a screen before and after eye operations, or used as visual teaching aids.

Charts and Plans

A time-saving method of producing statistical charts or floor plans with printed adhesive tapes and tabs allows reproduction by photostat or photo-offset. Charts are easily corrected or brought up to date. Kits available for each use include all the materials and tools necessary, plus a variety of tapes, tabs, and templates.

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Recorder

Oscilloscribe is a direct-writing oscillograph capable of recording chart speeds of 35, 100, and 200 mm/sec. At the latter speed each cycle of a 200-c/sec signal

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is traced out in a 1-mm interval. A. R. Eckels, of North Carolina State College, and I. S. Blumenthal, of Northrop Aircraft, reported at the winter meeting of the American Institute of Electrical Engineers that this very high-speed instrument would find wide application in electrocardiography, electrophysiology, servomechanism research and design, 60-c power system study, vibration and nuclear radiation measurement, and many other uses in the rapidly developing instrumentation field.

Free Sample

Fisher Combax combustion boats and covers, for use in holding steel samples during carbon and sulfur determinations, will not crack or burn through even at temperature tests above 2400° F. Sample packages are available while they last.

News from Palomar

Installation of new equipment on Palomar Mountain will make the Caltech auxiliary station there one of the best earthquake recording units in the world. Two electromagnetic linear strain seismographs will record strains to which the earth is subjected by seismic waves as small as one billionth of an inch per inch. A vectorial recorder, which will photograph a pattern roughly × 5,000 of the earth's surface motion in two dimensions, will make it possible to determine at a glance the kind of movement in a seismic wave. A tripartite seismograph will indicate quickly and accurately the direction from which earthquake waves and microseisms arrive at the station. In the Pasadena Laboratory a magnetic tape recorder will continuously record the earth's vibrations. When the tape is run through a playback machine at a speed of about 15 in./sec, the low-frequency vibrations are raised to a frequency at which they can be heard. Thus local shocks sound like pistol shots and distant quakes like a ten-strike in a bowling alley. All the new instruments were designed and developed by Hugo Benioff, professor of seismology.

Pat. Pending

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Field headquarters of the Juneau Ice Field Research Project. Research station on a nunatak at an elevation of 3875 feet overlooking Taku Glacier, 16 miles above its terminus, was built in 1949 and has been occupied throughout the summers of 1949, 1950, and 1951. Extensive meteoroglaciological observations are being made by the American Geographical Society under contract with the Office of Naval Research and with the active support of government and civilian organizations and private institutions. (American Geographical Society photo.)

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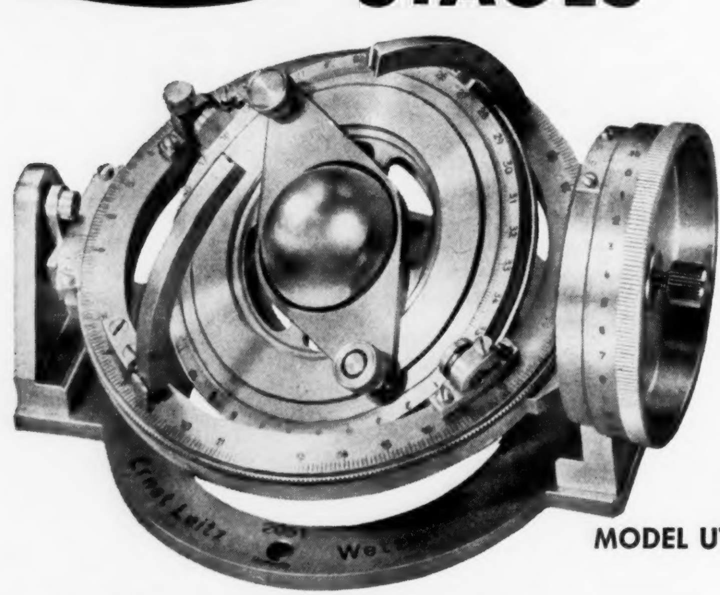

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THE SCIENTIFIC MONTHLY

MARCH 1952

The American Geographical Society: 1852-1952

JOHN K. WRIGHT

John Kirtland Wright (Ph.D., Harvard, 1922) has been a member of the staff of the American Geographical Society for thirty-one years. He has served as librarian, editor of certain special publications, research worker, and (1938–1949) as director of the society. He is a past president of the Association of American Geographers and past vice president and chairman of Section E (Geology and Geography) of the AAAS. His special interests have lain primarily in the border zone where geography and history meet. A volume by him, Geography in the Making: The American Geographical Society, 1852–1952, *is scheduled for publication next month.*

ONE hundred years ago American clipper ships were making record runs to Europe and the Far East, American whalers were penetrating icy seas, and American missionaries were going out to lonely Pacific islands and other remote places. From Mexico we had recently acquired an immense new domain, much of it little known, and over its dusty plains emigrant trains were wending their way, lured by California gold. All through the Eastern and Midwestern states construction gangs were at work building railroads and stringing telegraph wires. In clubs, in the lobbies of Congress, and in business houses men were wrangling over plans for carrying the rails across the continent to the Pacific.

New York City was the hub of this teeming national enterprise, and in New York geographical information was in demand as never before. The merchant, the missionary, the shipowner, the engineer, the railroad and telegraph promoter needed it for utilitarian purposes; the newspaper editor had to have it as background for the news; and the events of the day gave it a fresh interest for the lawyer, the doctor, the clergyman, and the scholar. Hence, on October 9, 1851, a number of New Yorkers of these diverse types met in John Disturnell's Geographical and Statistical Library, 179 Broadway, and organized a geographical society.

Several similar societies had already been established in other countries, after the founding of the Société de Géographie de Paris had set a pattern in 1821, and the men who gathered at Disturnell's must have felt it something of a reproach to their city and their nation that, in this respect, they had lagged behind the French, the British, the Russians, the Mexicans, and the Brazilians—even behind such German towns as Frankfurt-am-Main and Darmstadt. Following the example of the societies in Mexico City, Rio de Janeiro, and Frankfurt, they named the new institution "The American Geographical and Statistical Society of New York," and this title it retained until 1871, when the words "and Statistical" were dropped.

The new society made an auspicious and vigor-

ous start. Among its early leaders were Henry Grinnell, retired shipowner and leading sponsor of the arctic explorations in search of the lost Sir John Franklin expedition; George Bancroft, historian of the United States, who served until 1854 as the first president; Henry Varnum Poor, editor of *The American Railroad Journal* and initiator of statistical publications well known to investors today; and Cyrus W. Field, promoter of the Atlantic cable. Distinguished men who spoke before the society included Lieutenant Matthew Fontaine Maury, the oceanographer; Commodore M. C. Perry, the opener of Japan's ports; Horace Greeley, the publicist, whose subject was a proposed Pacific railroad as a means of enabling young men to "go West;" the arctic explorers Kane, Hayes, Hall, and Rae. During the fifties the society rented rooms, first in New York University and then in Clinton Hall; it built up its membership to somewhat more than 500, accumulated a library of some 5000 volumes, and sporadically published a journal under the titles *Bulletin* and *Journal.* It also sought to exert influence by holding discussion meetings, appointing special committees, adopting resolutions, and presenting memorials to the President or the Congress of the United States, cabinet members, and the state legislature, thus operating frankly as a pressure group. Among measures to which it gave support were the establishment of a department of geography in the Library of Congress, the improvement of the census, the inauguration of exchanges of American and foreign publications, the introduction of a decimal system of weights and measures, a Hudson–Lake Champlain ship canal, and even the restoration of the financial credit of the nation!

Although from the outset the society thus functioned as a national institution, it rested on none-too-stable foundations in its early years. It had no capital funds and depended for existence almost wholly upon membership dues and small gifts. In 1859 and 1860 it was torn by internal dissension, and during the Civil War the membership fell off and interest gradually waned. Had not Peter Cooper and the other trustees of the Cooper Union come to the rescue in 1867 by providing rooms rent-free in their building the society would probably have expired.

The recovery that followed this generous action may be ascribed largely to Judge Charles Patrick Daly, who was then president of the society and served in that capacity until his death in 1899. Son of poor Irish parents who had come to this country two years before his birth in 1816, Daly was a self-made man who was not only a distinguished judge, but a man of broad interests and of remarkable drive and enthusiasm in developing them. Alexander von Humboldt is reported to have said of him: "Few men have left upon me an impression of such intelligence on subjects of universal interest." His passion for geography may well have been aroused by three years spent at sea when he was a youth. Through voracious reading, extensive correspondence, and the frequent reception of travelers and explorers at his dinner table, he kept informed of geographical progress, and during the seventies and eighties he delivered before the society a series of annual addresses, in which he presented masterly summaries of the geographical work of the world. Known to the legal profession as an expert on the principles of evidence, he made good use of his knowledge and judgment in dealing with

West side of Broadway at Dey Street, 1856, showing site where the American Geographical Society was founded, at 179 Broadway (fourth building from the left).

controversial matters in the field of geography, and his geographical pronouncements had a critical quality that elevated them above those of the dilettante. He did much to establish the society's traditional devotion to sound, scholarly work.

Under Daly's thirty years of leadership the society prospered and gained in prestige both at home and abroad. In the early seventies the membership was enlarged to well over 1000 and remained close to that figure until 1916. In 1876 the society acquired a five-story brick house as its headquarters and, by 1901, good financial management, together with bequests, enabled it to erect a handsome stone building on West Eighty-first Street near the American Museum of Natural History. After 1870 the *Journal* came out regularly, six or eight popular lectures were delivered each year, crowded meetings were held in honor of arctic explorers, of Henry M. Stanley, of the Emperor of Brazil, and in memory of David Livingstone, and among the eminent men elected to honorary membership were the geographers Von Humboldt, Ritter, and Petermann, the explorers McClintock, Nordenskiöld, and Baker, the potentates Dom Pedro II of Brazil and Ismaïl Pasha of Egypt. A subtle change, however, gradually took place in the nature of the society.

As long as it had been small and poor, the members participated actively in its administration, major decisions were made by vote of the membership, and the council, or governing board, functioned as little more than an executive committee. When fortune began to smile more indulgently, the membership let the initiative and the responsibility pass little by little into the hands of the council, and, as the councilors grew older, something of the exuberant vitality of the earlier period was lost. After the mid-eighties no more large and impressive meetings were held, and only rarely did the society seek to exert pressure on the government in support of worthy measures, as it had done so often in its youth. Although one important step was taken in 1895, as we shall see, the last fifteen years of the nineteenth century formed one of the society's least enterprising periods.

Then came a transformation, soon after the turn of the century. The long process of aging reached its inevitable close, as deaths and resignations took their toll of councilors who had served honorably and well, if conservatively, during their latter years. By 1907 the society was being run by an almost wholly new board of relatively young men, and one of them, Archer Milton Huntington—a councilor to this day—was elected president in that year.

Charles Patrick Daly, president, 1864–99. Picture taken about 1869.

Scholar, poet, lover of Spain, and collector of Spanish works of art, Mr. Huntington had recently built the Hispanic Museum on upper Broadway. Recognizing the potentialities latent in the American Geographical Society—in its library and in the scholarly tradition Judge Daly had fostered and a gentle and devoted editor and librarian, George C. Hurlbut, had strengthened—Mr. Huntington took a number of decisive actions during the nine years of his presidency. Since the new house on Eighty-first Street was already proving inadequate, he made it possible in 1910–11 for the society to erect its present, much larger headquarters on upper Broadway, as one of a group of buildings in which the Hispanic Museum occupies the central position. To celebrate the completion of the new building and the sixtieth anniversary of the society's founding, he also enabled the society in 1912 to conduct a Transcontinental Excursion through the United States, under the leadership of William Morris Davis, of Harvard. Forty-three European and ninety American geographers took part in this unique and memorable trip, of which the benefits to the participants and the society alike were long-lasting. Also, through substantial contributions, Mr. Huntington helped the society enlarge and improve its collections, and through wise counsel he was largely instrumental in securing more effective collaboration between the society and the professional geographers of the country as represented by the Association of American Geographers (founded 1904). Furthermore, Mr. Huntington perceived that one of the chief weaknesses of the organization lay in its lack of coordinated administration, and he deserves much of the credit for the

appointment of Isaiah Bowman, then a professor at Yale, as its first director in 1915.

Dr. Bowman, then thirty-six, had gained a reputation as a scholar through the publication four years earlier of his comprehensive *Forest Physiography: Physiography of the United States and Principles of Soils in Relation to Forestry,* and had also impressed the council by the energy and efficiency with which he had conducted an expedition to the southern Andes in 1913 under the society's auspices. Appointed director during the dark hours of the first world war, he held this post for twenty years. Building on the solid foundations that his predecessors had laid, he brought the society's work into accord with the needs and interest of the new world, the new "Age of Troubles" that began in 1914. The times themselves raised a challenge: "How may geography be made to bear more usefully, more powerfully, than ever before on crucial human problems?" Under Bowman, the society met this challenge. Its program was oriented to shed the light of scientific understanding on contemporary world and national issues. At the same time sight was not lost of the values that spring from man's thirst for knowledge, irrespective of its immediate applications.

When the United States entered the first world war, Dr. Bowman placed the facilities of the society at the disposal of the government. From the autumn of 1917 until December 1918 the society's building was used as headquarters of the "Inquiry," a group of some 150 experts, whom President Wilson had authorized Colonel House to bring together to gather and prepare data for use at the coming peace negotiations. Bowman played an influential role as chief territorial specialist of the Inquiry and during the following year in various executive and other responsible positions on the American Delegation at the Peace Conference. When he returned from Paris in December 1919 he had greatly enhanced both his own and the society's reputation, and this facilitated his later successes in gaining scientific and financial support for his program for the enlargement of the society's work.

The spirit animating this program was thus expressed in a resolution adopted by the Council on March 20, 1920:

Resolved: That the Society regards its most important function as that of research and the preparation of original scientific matter which shall be of permanent value; and that the work of the staff will be directed in the first instance in accordance with this principle.

As director, Dr. Bowman brought together a staff of scholars and cartographic technicians. He received the harmonious support of an understanding board and of three men of distinction who served as presidents of the society: John Greenough (1916–25), John H. Finley (1925–34), and Roland L. Redmond (1934–47). He broadened the scope and deepened the intellectual and scholarly quality of the society's periodical, the name of which was changed in 1916, soon after his appointment, from *Bulletin of the American Geographical Society* to *the Geographical Review*. Through judiciously planned and skillfully managed campaigns, he increased the membership from 1100 in 1915 to nearly 6000 in 1930 and during the depression kept it from falling off as calamitously as was the case in many another comparable institution. The increase in membership produced substantial additional revenues and extended the range of the society's influence. Dr. Bowman also launched the publication of several series of monographs—notably the well-known *Research Series* and *Special Publications*—in which some eighty separate works have appeared to date (January 1952).

His most distinctive achievement, however, was the inauguration of three comprehensive, long-range programs of original investigation pertaining to the geography of Hispanic America, of the pioneer belts of the world, and of the polar regions. These were conducted in part by members of the society's staff and in part collaboratively by scientists in many different countries. The principal fruit of the Hispanic-American program, the most ambitious of the three, was the *Map of Hispanic America on the Scale of 1 : 1,000,000* (or "Millionth Map"), in 107 sheets, of which the first edition was completed in 1945 after twenty-five years of work. More than half of the funds needed for these undertakings was contributed by two members of the society's council, Mr. Huntington and James B. Ford, vice president from 1915 until his death in 1928. Generous support, however, was also received from other members of the board, from

The Transcontinental Excursion of 1912 visits Denver. "1—A triple alliance of foreign geographers. Left to right—Dr. Eduard Brueckner of the University of Vienna, Austria; Dr. Olinto Marinelli, Florence, Italy, and Dr. Joseph Partsch, professor of geography, Leipzig, Germany. 2—Prof. William Morris Davis of Harvard University, who is in charge of the excursion. 3—Edouard Alfred Martel, late president, central commission of the Paris Geographical society, and editor of La Nature. 4—Two Russian members of the party. On the left is Maj. Gen. Jules De Schokalsky, professor of the section of physical geography of the Imperial Russian Geographical society, St. Petersburg. On the left is Vladimir Doubiansky, conservator of the Imperial Botanical gardens at St. Petersburg. 5—Dr. Lucien Gallois, professor of geography, University of Paris. 6—Dr. Eugene De Cholnoky, professor of geography, University of Kolozvar, Hungary." —The Denver *Post,* September 26, 1912.

[Ge]ographers of Europe Amazed by Grandeur of the Mountain Scenery Revealed in Colorado

[ME]MBERS AND FOREIGN GUESTS OF THE EXCURSION PARTY OF THE AMERICAN GEOGRAPHICAL SOCIETY, WHO ARE IN DENVER WHILE ON A TOUR OF THE UNITED STATES.

Nothing Like It to Be Found in All Europe.

"Colorado's mountains and their scenic [illegible]lendor surpass anything that can be [illegible] in the Alps," said a score of Europe's [illegible] geographers, who arrived in Den[illegible] a special train over the Colorado [illegible] railroad last night from Grand [illegible]. Prof. André Chaix of the Uni[illegible] of Geneva, Switzerland, asked [illegible] could procure more films for [illegible] and when told that it would be [illegible]possible for him to get them in [illegible] time this morning, said: "Oh, [illegible] be without films in the famous [illegible] I cannot be without them

the Rockefeller Foundation, and from the Social Science Research Council. Although not initiated by Dr. Bowman, the School of Surveying, established at the society in 1922 by Alexander Hamilton Rice, should also be mentioned. Maintained and directed by Dr. Rice until 1932, the school served chiefly as a research unit in the fields of exploratory techniques, cartography, and photogrammetry, and was succeeded in the thirties by two research departments of the staff.

In 1935 Dr. Bowman resigned as director to accept the presidency of The Johns Hopkins University. He continued, however, as an influential member of the society's council until his death in 1950. During this period no essential changes were made in the over-all policies of the institution. J. K. Wright served as director from 1938 to 1949.

At the time of the second world war the society again placed most of its facilities at the disposal of the government. Some forty different agencies of the government, and private organizations working in its behalf, made use of the collections. Their representatives consulted, copied, and microfilmed books, maps, periodicals, and catalogues, and many works from the society's library and map room were lent to New York and Washington offices. Under a contract with the State Department, which remained in force for three years, work was done on some eighty projects, large and small, most of them of a cartographic nature. With government aid the society was able to publish several new maps in color, notably a *Map of the Americas, 1 : 5,000,000,* embracing the two American continents in five sheets, and a map of the world on the Miller cylindrical projection (equatorial scale, 1 : 30,000,000).

Early in the forties two long-range programs of research were begun, one in the relatively unexplored realm of medical geography, the other concerning the glaciers of the Western Hemisphere. During the war little progress could be made upon them, but in 1948 they were revived and are now being carried ahead by special departments. The medical program calls, among other things, for a general stocktaking of what is already known in the field, for intensive studies of the geographical pathology of one or more regions of limited area, and for investigation of a selection of specific diseases as distributed over the face of the earth. The last is taking form in the production of an atlas of diseases, now well under way. Four large sheets in color have appeared in the *Geographical Review,* presenting maps covering poliomyelitis, malaria, cholera, and helminthiasis, and some twenty more are in preparation or being planned.

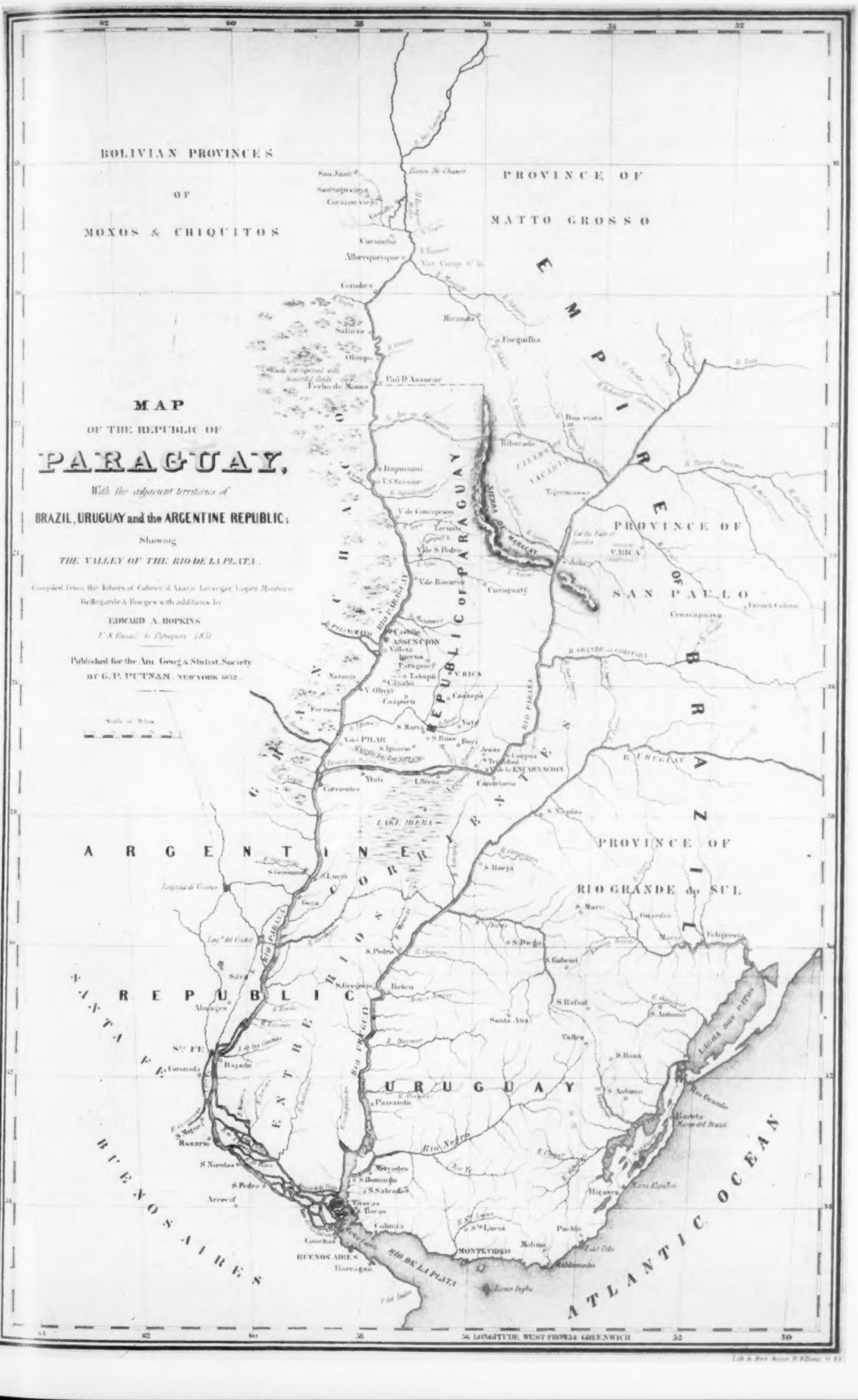
The glaciological program also involves a general and continuing inventory of research completed and in progress elsewhere and detailed investigations in particular areas. Scientific parties have worked on the Juneau Ice Field in southeastern Alaska every summer since 1948, and a reconnaissance study has been made in Patagonia. Both the medicogeographical and the glaciological programs are receiving cooperation from governmental agencies and other scientific institutions.

During the late forties, under the guidance of Richard U. Light, who had been elected president in 1947, the society undertook a thoroughgoing review and appraisal of its policies, program, and outlook. The council and the staff made intensive studies of the institution's problems, and several score geographers and educators were consulted at a series of conferences. Out of the comments and suggestions thus obtained, the council formulated a declaration of guiding principles, which the society is now following under the leadership of George H. T. Kimble, an Englishman of vision and ability who was appointed director in 1950, after having served for five years as head of the Department of Geography at McGill University. The society's function as a research institution is to be maintained, strengthened, and enlarged. At the same time a frontal attack is to be made upon the geographical illiteracy and ignorance so dangerously prevalent among the American people today, and, as one means of attack, *Focus,* an attractive new periodical, was launched in the autumn of 1950. Issued ten times a year, it is designed to provide background facts and geographical interpretation of current news for a wide and diverse audience.

Such, in broad outline, has been the society's record as an institution during its first hundred years. What of the geography that it has sought to develop and disseminate?

The trends in the relative number of papers bearing on different parts of the world that appeared in the society's periodical from 1852 through 1950 are shown in the accompanying graph. These trends were controlled partly by shifts in the tides of geographical interest on the part of the American public and partly by changes in the special interests of the society itself. The ups and downs in the graph for the United States are particularly

The first map ever published by the American Geographical Society. It appeared in Vol. 1, No. 1, of the *Bulletin of the American Geographical and Statistical Society* (Aug. 1852) and illustrated a paper on Paraguay by Edward A. Hopkins.


MAP
OF THE REPUBLIC OF
PARAGUAY.
BRAZIL, URUGUAY and the ARGENTINE REPUBLIC;
Showing
THE VALLEY OF THE RIO DE LA PLATA.
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LONGITUDE WEST FROM GREENWICH

interesting. Before 1875 this country received considerably more attention than any other part of the world. This may be attributed during the fifties and sixties to the members' special interest in domestic issues, which manifested itself not only in the pressure-group activities to which I have referred, but also in a demand for enlightenment on various unfamiliar aspects of the American scene. During the early seventies, on the other hand, the progress of explorations west of the Mississippi accounts for about three quarters of the papers on this country.

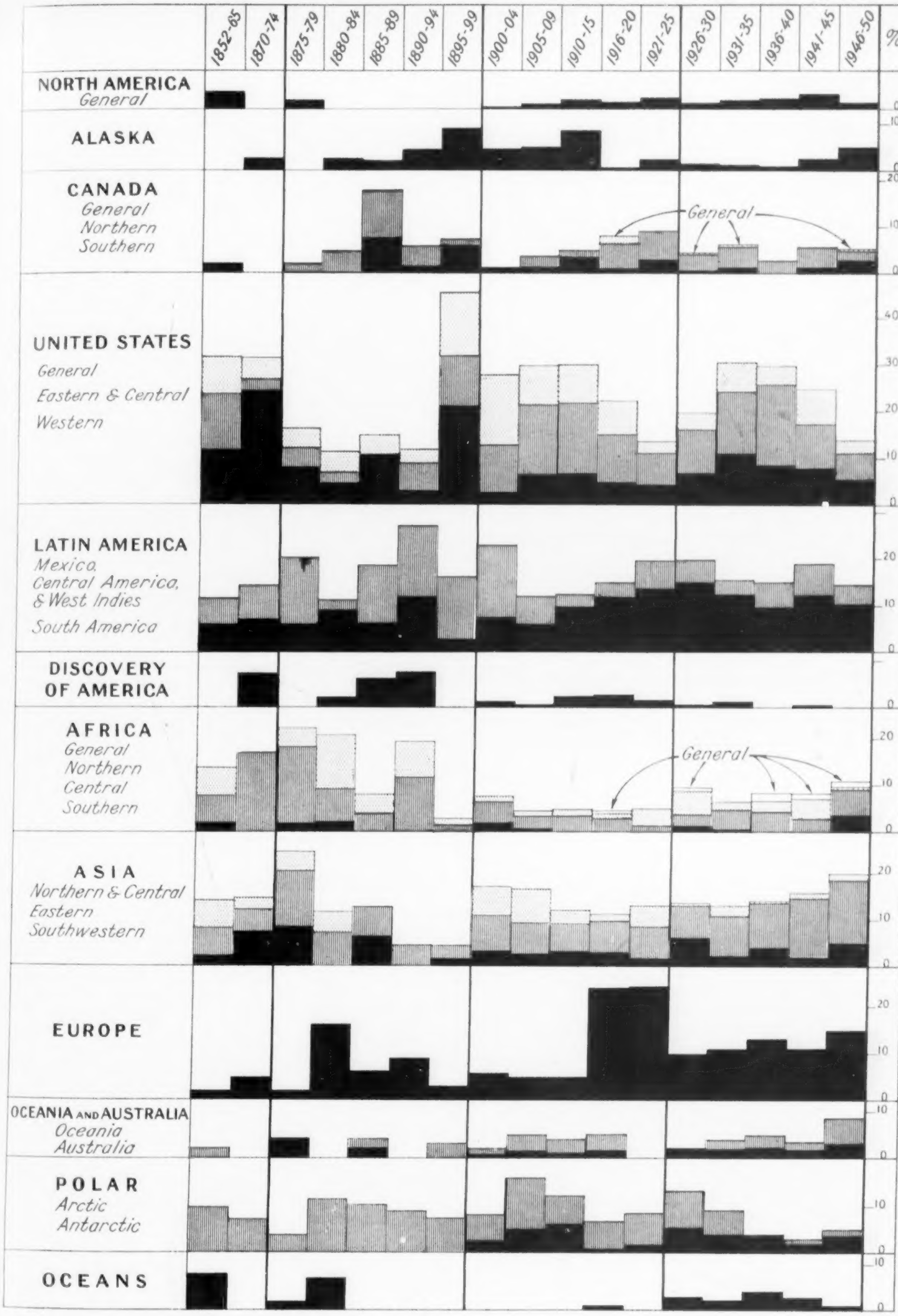
After 1874 the tide of interest ebbed away from the United States, over which it stood low for twenty years. This was the heyday of Judge Daly's administration, and the wider world outlook that he brought to the society goes far toward explaining the shift to Africa, then being opened up, and to Latin America and Asia. Between 1895 and 1900, however, the tide flooded back so deeply over the United States and Alaska as to leave much of the rest of the world almost bare. In no other five-year interval was this country so disproportionately emphasized, and, although the balance was partially rectified after the turn of the century, some 30 per cent of the papers concerned the United States from then until the coming of Dr. Bowman as director in 1915.

The stress laid upon the United States between 1895 and 1915 was due chiefly to the emergence of a geographical profession in this country under the leadership of William Morris Davis, Albert Perry Brigham, Ralph Stockton Tarr, Richard E. Dodge, Henry Gannett, and others. Most of these men were teachers in colleges and universities, and they might well have accepted "Home Geography First" as their slogan. Writing in 1894 to the publisher Henry Holt, who was then a member of the society's council, Professor Davis suggested that the society should interest itself in "home" rather than in polar exploration, and that "first and foremost" it should promote a geographical survey of New York state. In a paper published in the *Bulletin* the next year Professor Brigham wrote: "The teacher of Physiography has no greater reward than is his when a student assures him that henceforth his Native State will be to him a new country, or that he shall see the hills and the valleys of his old home with new eyes."

Until the mid-nineties the society had not paid much attention to the teaching of geography. It had published only two papers bearing exclusively on the subject, both by Konrad Ganzenmüller, of Dresden. One argued that geographical instruction should be "enlivened" and "lightened" by teaching students the meaning of geographical names (e.g., *Kynoskephalai* = "Dogs' heads"), and the other provided the wherewithal for so teaching them, in the form of a glossary. Ganzenmüller's proposal, however, did not strike at the core of the problem of geographical education. In 1894 the society's council appointed a special committee, of which Henry Holt and Cyrus Cornelius Adams (then a councilor and later editor of the *Bulletin*, 1909–15) were members, with instructions to consider ways of "bringing the Society into closer relations with geographical observers, writers, and educators," and from the recommendations of this committee came a change in the character of the *Bulletin*. By adapting its periodical to the needs and interests of teachers, the society heightened its pedagogical value, but the breadth and a certain urbane catholicity that had characterized the early numbers were sacrificed, and the *Bulletin* became "Americo-centric."

In the *Geographical Review*, which succeeded the *Bulletin* in 1916, a wider outlook and a better balance were restored and maintained. From 1916 to 1925 interest in the first world war, and in the problems of reconstruction that followed, brought the ratio of articles on Europe up to about a quarter of the total. Many of these articles were secured as a direct result of the contacts that Dr. Bowman had established with European geographers and others during the Transcontinental Excursion of 1912, and later at the Peace Conference. After 1925, however, there was a sharp decline in the relative amount of space given to Europe, and, when the depression in the thirties drew the attention of American geographers predominantly to the study of national and regional problems at home, the United States resumed the lead. The downward trend in the curve for the United States in the forties resembles that of the years 1915–25, and for like reasons. A great world war turned the tides of interest to the lands around the Pacific and, though to a lesser degree, once again to Europe.

When we turn from the areas and topics of geographical investigation to its nature, methods, and purposes, we enter a domain in which the progress of a century cannot readily be summed up in a few words or presented in graphs. Be it said, however, that the American Geographical Society has never been overmuch concerned with precise definitions of geography, or unduly troubled by where its metes and bounds should be drawn. In 1925 Dr. Bowman wrote: "What a subject should be in content or style of treatment is not a matter of final and definite decision by a court



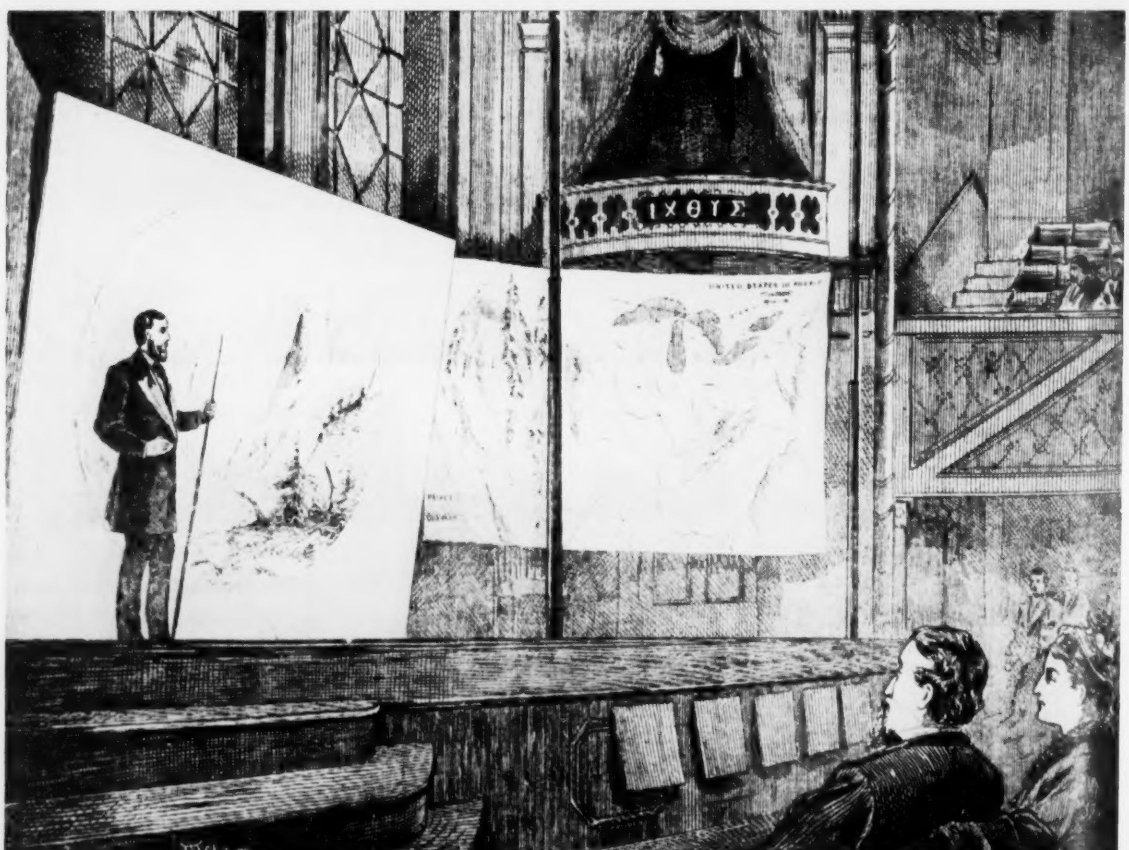
Trends in the relative number of regional articles in the Society's periodical relating to different parts of the world, 1852–1950. For each period shown the combined length of the several bars in the column equals that of the bars in each of the other columns, and each sum represents 100 per cent of the specifically regional articles that appeared in the period.

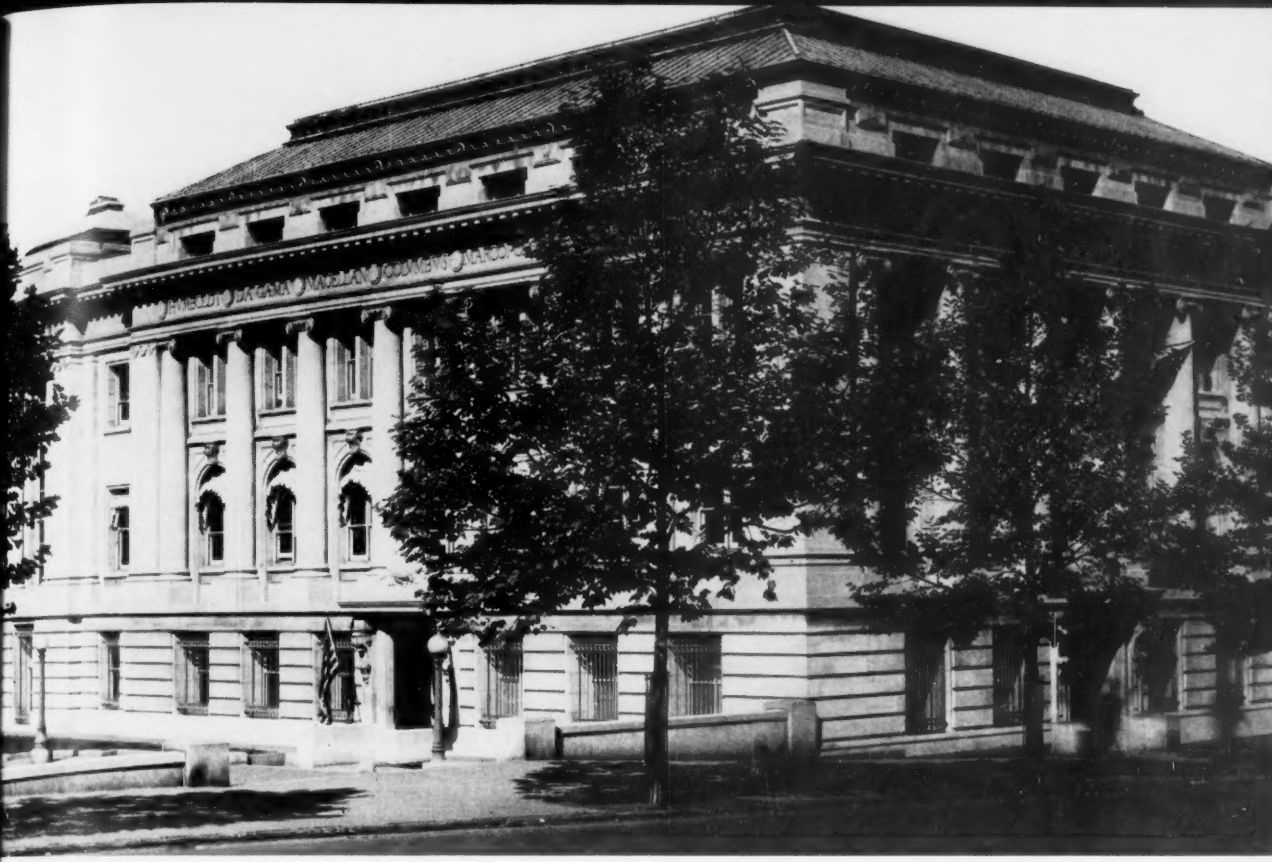
of reference, especially if that subject deal with man, his ways and his works," and the society has adhered to this precept.

The methods of geography involve the fundamental procedures of gathering, presenting or portraying, and interpreting data. The society's publications reflect the prodigious strides that have been made since the eighteen-fifties in the first two of these, owing largely to technological advances. Aerial photography and the art of photogrammetry, to which the society has made conspicuous contributions since 1915, have rendered possible the assembly of the raw materials of geographical knowledge with a speed and precision beyond the wildest dreams of even the most imaginative geographer of, say, the year 1910. Almost equally impressive have been the improvements in the portrayal of geographical information by means of maps and photographs and the broadening of the scope of the information so portrayed. Standards have also been raised with regard to the conciseness and coherency of textual exposition and description, though this gain has been achieved at the expense of a vividness, force, and picturesqueness of style found in many of the papers of the earlier period.

The accumulation and descriptive presentation of facts pertaining to the distribution of phenomena on the earth's surface have always been and will continue to be a large part of geographical endeavor. It is not, however, so much an end in itself as the means to the larger objective of explanation, interpretation, and use—as W. M. Davis strove long to impress upon his colleagues. However, along with the gain in the proportion of papers in the society's periodical that seek to explain as well as to describe, there would seem to have been a corresponding decline in the exuberance of the explanations. The geographers of today do tend to be more cautious. The superabundance of known facts and the mores of contemporary science inhibit them from magnificent, sweeping generalizations of the kind in which some of their predecessors felt free to indulge.

Professor F. V. Hadyn giving an account of his explorations in "The Great Northwest" before the American Geographical Society, April 15, 1874, at Young Men's Christian Association Hall. From *The Daily Graphic,* April 20, 1874.





Present headquarters of the American Geographical Society at Broadway and 156th Street. Building erected in 1911.

From the beginning the society has sought to accomplish four purposes: the advancement of geography as a science, as an educational discipline, as a guide and instrument of practical use, and as a means of bringing delight through its appeal to the imagination. The last, no less worthy than the other three, though stressed only during the first fifty years, has never been neglected. The educational purpose, in the specific sense of a concern for the improvement of the teaching of geography in schools and colleges, did not become an influential factor until after "the turn of the tide" in 1895. The advancement of geographical science and the practical use of its findings have always been the predominant aim of the society.

Under dynamic leadership, and drawing inspiration from its traditions of a hundred years of scholarly integrity and breadth of interest, the society is now meeting the challenge of its second century with confidence and vigor.

Economic Mobilization: Problems and Prospects*

MARION B. FOLSOM

Mr. Folsom has been with Eastman Kodak Co. since 1914, and treasurer since 1935. His extracurricular activities as chairman of the Board of Trustees of the Committee for Economic Development reflect his long-time interest in economic problems. He was director of the House of Representatives Special Committee on Postwar Economic Policy and Planning, 78th–79th Congress, a member of the President's Advisory Council on Economic Security (1934–35), and a member of the Federal Advisory Council on Social Security (1937–38). He has also served on many other committees having to do with industrial relations.

THE defense program, which was forced upon us by the militance of Communism and by our failure to maintain our armed strength at the end of World War II, is the dominant force in our economy today. The way we deal with the problems it raises will determine in large part both our security from foreign aggression and the health and strength of our domestic economy.

The many problems raised by the defense program can, I think, be divided into two main categories. First are the problems imposed on the economy by the existence of the rearmament effort. In this category are problems of production, the danger of inflation, the possibility of impairment of our living standards and our freedom, and a number of problems connected with our relations with other free nations. Allied with these are the problems of transition after the peak of rearmament has passed.

The second main category of problems relates to the defense effort itself. What is the real nature of the threat to our national security and that of the rest of the free world? How big should the defense program be? How rapidly should we rearm? What form should our military defenses take? To what extent should we rely on military measures and to what extent on nonmilitary means in our security program? After one and a half years of defense, there is still much confusion in this area.

I shall discuss these two groups of problems and what the Committee for Economic Development thinks should be done about them. Begun as a temporary and experimental organization, the CED is now rounding out its tenth year of operation. The committee is no longer temporary, and we feel that the experiment has been a continuing success. When the original task, which was to study problems of maintaining high employment after World War II, was completed, we all felt that we could make further contributions in that and other economic fields. Basically, our methods are simple: the CED brings together a group of its businessmen trustees—the Research and Policy Committee—and a group of social scientists, mostly professors of economics, which is called the Research Advisory Board. These two groups study and discuss together the problems we have selected for research. Basic research, when needed, is done by scholars expert in the fields being considered. The final result of this joint process takes the form of statements on national policy prepared by the Research and Policy Committee. Because the CED has conducted its research on an objective basis and oriented its program toward long-range problems as well as those of a shorter-run nature, we feel that we can make a relatively lasting contribution both to current thinking and to the preservation of our way of life.

The joint analysis of economic problems is an important feature of the committee's method of operation. We businessmen have found that we can learn much from the scholars. Academic economists are apt to have a more objective and longer-range viewpoint than most business people. And, I believe, the scholars have also learned something from us about the practical working of our econ-

* Based on an address before the National Academy of Economics and Political Science, Annual Meeting, AAAS, Philadelphia, December 27, 1951.

omy. An important by-product of CED's work is the mutual respect that has grown up between the businessman and the academician. It has been continually gratifying that we can educate each other, get on a common footing of understanding, and then pass on some of this understanding to the general public.

Since the outbreak of war in Korea, the CED has naturally turned its chief attention toward economic policies for the defense program. The key to the success of the defense program is production. Although production of needed military equipment for ourselves and our allies should take first priority, we are not engaged in an all-out war. In World War II, military production accounted for 45 per cent of our gross national product. At the peak of the present program of rearmament, current plans call for about 20 per cent of total output. In this part-war, part-peace situation, it is essential that civilian as well as military production be sustained. To accomplish this dual production task, output must be increased wherever possible. This calls for new production methods, greater operating efficiency, and substitutes for scarce materials. New skills should be developed in the labor force through training programs, and more women should be attracted into industry. Waste should be reduced. New plants for essential production, longer working hours, fewer breakdowns and work stoppages—all these things are required if production is to be increased.

But greater production alone is not sufficient. The defense program has changed the pattern of production, and it requires shifts from civilian lines to military output. In some industries this can be accomplished more easily than in others, but it is inevitably a difficult process. Moreover, the shifts of production are reflected in changed demand for basic materials. Bottlenecks develop which must be broken.

Closely connected with the need to increase and rechannel production is the problem of maintaining and increasing productivity during the rearmament period. Since 1900, output per man-hour has almost quadrupled; there has been a historical rise in productivity of 2.5 per cent a year, on the average. Obviously, the needed rise in total production would be fostered by rapidly rising productivity. But the impact of a sharp economic change is likely to impair the chances for higher output per man-hour.

During World War II, in spite of the vastly increased rate of production, productivity in the economy as a whole actually decreased. The reason is not hard to find. The transformation of the economy from almost total devotion to civilian pursuits to a condition in which one half our productive effort went toward satisfying military needs left in its wake a multitude of maladjustments, shortages, and bottlenecks. Investment in new plants and equipment, which is primarily responsible for subsequent increases in productivity, went almost entirely into war production. Great numbers of young men who would ordinarily have been in the civilian labor force were serving in the armed forces. Inexperienced and marginal workers lowered the average level of efficiency in the wartime labor force. Despite some spectacular cases of increased productivity, the average for the nation declined, and it did not begin to rise again until after the war.

This historical fact leads directly to the question: Will the same thing happen in the defense program? The answer is not readily at hand. But it seems possible to draw, from a comparison of the wartime situation and the present, the conclusion that it need not happen. The dislocation of the economy during the war was far more extensive than it will be during a rearmament program of the size now contemplated. Most civilian production will continue at extremely high levels. Military production will be enormous by normal standards, but it will in no way approach the size relative to total production that would be expected in a full-scale war. Years of prosperity have left industry, commerce, and agriculture generally with means with which to lay the groundwork for increases in productivity. The armed forces are to include only one third as many men as during the war, and in the meantime the total regular labor force has increased by several million persons. It is not only possible that productivity will continue to rise, but also that after the rapidly expanding phase of defense it will enjoy a spurt.

Increases in productivity determine increases in the standard of living in normal times, when defense expenditures are relatively low. But with a defense program running at what will soon be a rate of $1400 per family per year, conditions are naturally different. So far, however, the standard of living has risen, on the average. In the eighteen months since the Korean War began there has been a great increase in private expenditure, both for consumption and for investment. The defense program, on top of a thriving civilian economy, has added more to personal incomes in the aggregate than taxes have taken away, and total incomes after taxes have risen more than prices. Many individuals, however, have seen their standards of living decline as taxes and the cost of living have

risen more than their incomes. Others have improved their economic status, increasing their incomes by more than the rise in taxes and living costs.

We are now at the stage in the defense program where defense expenditures will probably rise more rapidly than the gross national product as a whole. It is unlikely that living standards will rise during the remainder of the defense build-up—they may, in fact, decline somewhat. The decline will be greater than the published figures will indicate. In part it will take the form of poorer services—crowded schoolrooms and hospitals, poor roads and other postponed public improvements. But the period of rapid expansion of plant capacity will be reaching an end.

The next twelve months, which should, according to current plans, see us past the peak of the defense effort, will be less a phase of investment than of production. Personal incomes will undoubtedly remain high, and consumption expenditures will probably also be high. The supplies of nondurable goods should be ample. Many so-called hard items like automobiles and refrigerators will be less plentiful, although people will still have the durable goods they have acquired in the past year and a half. And when the defense program tapers off, many people will have money with which to replenish their supplies. But a resurgence of inflation can change this picture. It can cause marked reductions in living standards for large groups in our population. It can also interfere seriously with our attempts to rearm, to increase civilian production, and to achieve a satisfactory rate of productivity gains.

The CED continues to believe that inflation is a great threat to the success of the defense program. From its beginning, the CED has devoted a great share of its research to the problems of economic stabilization. Since World War II, it has become increasingly clear that the greatest contribution to economic stability can be made through policies and actions designed to prevent inflation. Since the beginning of the war in Korea, the CED has repeatedly drawn on its earlier research on economic stabilization and has offered a program for the control of inflation fitted to the current situation.

In addition to measures designed to increase total production, our basic anti-inflationary program calls for a balanced set of policies intended to reduce the excess of demand over civilian supply. This program includes four major fields of action. First, we have called for strict economy in government expenditure, federal and local, nonmilitary and military. We have recommended also, as part of this policy, the postponement of all deferrable government expenditures. Despite its importance, however, and despite the universally voiced support for economy in government, efforts to date have been disappointing. The CED feels that there must be ways to make this vital problem more than a topic of conversation, and we have recently begun a study of it. We intend to go into the budget-making process in detail, from the stage where an initial demand for government spending arises to the point where Congress authorizes expenditures and the government starts to spend the money. We hope to make a lasting contribution, but we have no illusions as to the ease of the task or the amount of time and study it will take.

Second, our program calls for taxation that will limit the rise in incomes available for consumers and businesses to spend. We believe that it is better to increase taxes than to have inflation. To this end, CED recommended substantial tax increases for both fiscal 1951 and fiscal 1952, together with specific programs for expenditure reduction and deferral. Now that the President's budget has been issued, we plan to issue a policy statement recommending a tax and expenditure program for fiscal 1953.

Thus far in the defense program we have been running cash budget surpluses and, according to present estimates, the cash budget will be approximately in balance for the present fiscal year. Our budgetary position undoubtedly did a great deal to stabilize prices in the last nine months of 1951. But it is difficult to be optimistic about the future. If military expenditures increase in accordance with present plans, the deficit for fiscal 1953 will be substantial. Unless expenditures are held below present estimated levels, or taxes are further increased, the cash deficit for the next fiscal year is likely to be 8–12 billion dollars. A deficit of this size would, of course, have a strongly inflationary influence. On the other hand, taxes are already at extremely high levels, and further increases involve the danger of serious long-run damage to the economy.

With the existing high taxes on individuals, particularly in the upper income groups, the incentive to invest is much less than in the past. The present rates are close to, if they have not already passed, the danger point. With the present high rates on corporations, not only is the incentive to invest lessened but the sums actually available are greatly reduced. If the large sums required to finance the plant and machinery needed to increase our capacity and to improve productivity are not avail-

able, our future progress will be greatly retarded.

The problem of developing a tax policy that will help control inflation while minimizing undesirable economic consequences requires consideration of the kind of taxes to be imposed as well as their amount. Increasing the excess profits tax, for example, will have little, if any, anti-inflationary effect, whereas it will increase waste and reduce future productivity gains. Every effort should be made to hold down expenditures so that no additional taxes are necessary this year. But if additional tax revenues are to be raised, maximum anti-inflationary effects with minimum damage to incentives would be achieved by some form of excise tax on a reasonably broad base. In view of its undesirability as a permanent part of a normal peacetime tax structure, it would be wise to enact such a tax on a temporary basis, limited to the duration of the emergency. Such a tax would be best imposed at the retail level, in the form of a sales tax. It should exclude food, housing, fuel, utilities, and certain items difficult to tax. Articles already subject to excise taxes should not be subjected to this sales tax.

Our third basic recommendation calls for monetary, credit, and debt-management policies to restrict the volume of credit and the money supply. If the money supply does not increase, the danger of excessive demand is greatly reduced. Monetary policy, to a greater extent than other measures for the restraint of inflation, has the advantage of flexibility. If administered alertly, it can be applied on short notice to meet any economic change. If restrictive monetary and credit policies had been imposed immediately upon the outbreak of the Korean War, the chances are great that we would have had considerably less inflation than has taken place. The effectiveness with which restrictive monetary policies have been employed by the Federal Reserve since last spring has demonstrated, in our opinion, the great value of these instruments for economic stabilization. We should do nothing to undermine the authority of the Federal Reserve Board or its freedom of action in the control of credit and the money supply.

A properly conceived and executed debt-management policy can also do much to prevent the spread of inflation. Clearly, the government should seek to finance the public debt, when refunding old obligations or issuing new ones, as far as possible by the anti-inflationary sale of securities to nonbank investors.

The fourth part of our program calls for a vigorous national campaign to promote savings of all kinds. In its governmental aspects, this is a task for the Treasury Department, and among the various measures that could be employed to prevent inflation the savings bond campaign has been least effectively used. After the buying sprees of 1950 and early 1951, private savings began a sharp rise. This was an important factor in the halt to the inflationary spiral we have been in since last winter.

Although savings rose rapidly, the sale of savings bonds did not increase. More E bonds have been cashed in this year than have been sold. The poor results of the 1951 campaign are sufficient evidence of the need for a new bond. It is hard to sell a 1941 model of anything in 1951. This is especially true of a bond, because of the marked change in interest rates during the past ten years. To be attractive, the new bond should bear a higher interest rate, particularly during the first few years after purchase. An intensive campaign with a bond of this type should greatly stimulate savings. If the campaign is successful it would also ease the debt-management problem of the Treasury.

Price and wage controls are also being used as a weapon against inflation. These controls, in our opinion, should be temporary and should play a supplementary role to monetary, fiscal, and savings policies in the present situation. In December we issued a policy statement entitled "Price and Wage Controls," in which we sought to analyze the effects of these controls on the economy. We concluded that on balance it was doubtful whether the harmful effects of price and wage controls did not outweigh their contribution to the control of inflation in a rearmament program of the size and type now planned. In particular, we believe that the system of controls that the authorities are now establishing, if continued, will do great damage to our economy. This system establishes a hold-the-line policy for business prices and farm prices above parity, but permits wages to rise with the cost of living, and farm prices below parity are exempt from control. In a period when inflationary pressures are strong, the cost squeezes such a program would produce would impair our goal of greater production of needed items.

For the limited period in which price and wage controls are in effect, we have proposed a "flexible-adjustment" policy in which the impact of the controls on different economic groups would be comparable. Under this system, price and wage ceilings would be adjusted upward or downward to match changes in costs—business costs, farming costs, and living costs. To bring about added restraint of inflation, these adjustments would be made only after a time lag of, say, three months. During the lag period, cost increases would have

to be absorbed. The CED believes that such a flexible adjustment system can help to restrain inflation with a minimum of harm to production while the more basic anti-inflationary measures are being strengthened.

I have spoken thus far of domestic problems. Important international questions also call for attention. Some of these, such as the rapid recent increase in inflationary pressures in West European countries, are in large part caused by the current rearmament program. Others, such as the recurrent balance-of-payments difficulties in Britain, have deeper roots and will extend well beyond the rearmament period. Still others, although they have an important bearing on the present defense emergency, are primarily long-run in their implications.

We must also face the problems of promoting economic progress in underdeveloped areas. The lives of millions of people in the free world are dominated by despair, fear, disease, and hunger. They look forward only to continued poverty. The Communists are promising these people more food, better housing, and some of the comforts of life. We know, because experience has demonstrated it, that the Communists cannot deliver on these promises and that they are mainly propaganda. There is a great opportunity for the rich and powerful nations of the free world, particularly our own, to take up the challenge and seek to help these people achieve real freedom and improve their productivity and living conditions. A program of assistance to underdeveloped nations is a long-run problem. It is a positive and a constructive program.

Such a program would be in our own interest. The world raw materials situation is becoming steadily more acute, and many of the economically backward nations have great untapped supplies of vital resources. As production of these materials becomes greater, we will have greatly expanded sources of supply. Meanwhile, as living standards and production increase in the underdeveloped areas, these countries will become better customers for American products, and when it is realized that the average per capita income in these countries is equal to about $80 a year the opportunities for improvement become obvious. Politically, one of the most valuable contributions to the security of the free world can come from improved living conditions in the backward countries. We will be showing that our activities in their behalf are concrete—unlike the Russian promises.

A program of assistance to underdeveloped areas should be based on economic development, not on relief. Our purpose is to teach skills, to lay the foundation for better health and transportation, and to make it possible for living standards to rise. Some of the programs already put into effect have achieved remarkable success with the expenditure of very little money. Particularly effective has been the work of trained technicians in the fields of sanitation, public health, and agriculture. Much more of this sort of activity should be undertaken. This kind of assistance helps the people of underdeveloped countries get a start toward helping themselves.

Many large capital improvement programs will also have to be undertaken, and whenever possible they should be undertaken with private capital from the wealthier nations. One of the major problems facing the free world is the establishment of conditions that will attract private capital to underdeveloped countries. A program of this sort will, in the long run, provide more security than military strength. Moreover, it will increase the wealth and the possibilities for expanded commerce in the world. It will be a real answer to the false promises of Communism.

According to Defense Mobilizer Wilson, the defense program will reach its peak impact upon the economy toward the middle of 1953. At that point, defense expenditures are expected to amount to about 20 per cent of the gross national product. After the peak has been passed, defense needs will account for a smaller share of total output, and also for less money in absolute terms. But we shall continue to devote a very large share of our resources to maintaining and keeping up to date the military machine we will have developed.

The transition from the peak of the defense program to the "maintenance economy" will, it appears, be far different from the postwar transition of 1945 to 1946. Then we cut back as rapidly as possible from the huge wartime military expenditures. Deferred demand was enormous, however, and American business promptly went to work to satisfy it. The tripled money supply laid the foundation for a sharp inflation. After 1953, conditions will be dissimilar in many respects. Military output will presumably continue high. The backlogs of deferred civilian demand will presumably be smaller. The money supply will probably not have increased on the wartime scale.

It is too early to judge what the economic developments of the post-rearmament period may be. We may face a period of deflation; business and consumer demand may decline when government purchasing falls off. Or we may make the transition quickly and experience renewed inflation; private investment and high civilian consumption may

more than fill the gap created by the modest cutback in military expenditure.

Neither marked deflation nor marked inflation is unavoidable, however. I am convinced that the prevention of large economic fluctuations is feasible. We already know much about the prevention of wide swings in economic activity—knowledge that has been painfully acquired. Through the application of wise public and private economic policies we can maintain a reasonable degree of economic stability without sacrificing other economic objectives. The problems will be much easier if we soon begin to level off defense expenditures and spread the program over a longer period, rather than reach a higher peak with a sharper cutback. What is important now is that we restudy the problems of stabilization in the light of new facts and new knowledge of the tools that are available for our use. CED has initiated such a study. It is my hope that other organizations will also attack this problem.

My second category of problems is that relating to the defense program itself. Here there are few answers and many questions. But it will be tragic if the answers are not found. The existence of a serious threat to our security is clear, but its nature and magnitude are hazy. What is the true threat to our security? In an all-out war the strength and ultimate objectives of both sides are relatively clear. But in the present situation there is a terrifyingly complex strategy problem. Our knowledge of activity behind the Iron Curtain is sketchy at best. In this new kind of war, the aggressors do not show their hand, and the defenders must try to figure out what cards they hold.

The CED is attempting a study of the threat to our national security, as we believe that this job must not be done entirely by the government. The public must assess the plans of the officials it has charged with the execution of the program. I should like to give you some of our preliminary thoughts on this subject. The first stage of analysis is an estimate of Soviet aims and capabilities. What do the Russians hope to accomplish against the free world, and particularly against the United States? The conclusions drawn from an assessment of Soviet aims and resources must not be one-time conclusions, but must take the form of continuing reappraisal. They must allow for changes in plan made by the Russians for the purpose of making our decisions obsolete and unsuitable. Our defense must be as flexible as the Russians' offense.

Having made an assessment of Soviet strength and goals, a comprehensive plan for the defense of our security must be developed. How much defense do we need? Our immediate goal is to deter the Russians and their satellites from attacking free nations. Both objectives are important in designing our defense. But the total size of our defense effort is only one of many considerations. National security depends not only upon military defense, but also upon a healthy economy and the preservation of individual freedom. It depends not alone on the size and efficiency of our armed forces, but also on the effectiveness with which we employ nonmilitary security measures—diplomatic, psychological, and economic. If we devote too large a share of our resources to weapons production, the economy and our long-run security will be harmed. On the other hand, if we try to get along with too few weapons, our immediate security will be just as surely impaired.

The United States is not alone in the defense against militant Communism. The other free nations have an important role in the security program. Each of them has its own set of special problems. How much should our allies contribute? How much of their contribution should be in manpower, how much in weapons, and how much in raw materials? How much military or quasi-military defense can they add to the total without impairing their precarious domestic economies? How much defense aid should we give them, and what proportion of our aid should be military and what economic?

How much should we be doing to help the underdeveloped countries improve their living conditions and production? In our zeal to build up military defenses against Communism, are we devoting enough of our thoughts and efforts to developing a positive and constructive program for lasting peace?

With respect to the military side of the security program, other questions must be asked. Are the military authorities planning their forces and weapons to fit the sort of war we might have to fight? How much of our military expenditure is being devoted to weapons we might be able to use and produce currently or on short notice and how much to designs for the future?

Clearly we need a balanced security program. Equally clearly, the ultimate responsibility for the defense of the free world rests on the United States.

It is of the utmost importance that the defense authorities try to establish a defense program based on a realistic assessment of the threat to security. Without an unbiased study of the threat and the program devised to meet it, the public has no way of knowing whether this requirement has been met. But even assuming that the program is properly

conceived, there are further doubts that must be resolved. Is there a coordinated plan tying together the different sections of the security program—military, economic, and political, national and international? Are military demands attuned to economic possibilities? Are the plans and schedules that have been drawn up being met?

Obviously, sensible public appraisal of the success of the defense program depends upon a constant flow of information. Our individual freedom is menaced and our national security jeopardized when information from our government to the public begins to turn from a flood to a trickle. It is necessary for those in charge of the defense effort to conceal from the public many facts that would be of benefit to the Soviet Union. This makes the task of disseminating information difficult. And when the information we do get comes to us in conflicting form, our ability to judge and appraise it is greatly impaired, as is our confidence in those who provide it.

Two years ago the CED issued a policy statement entitled "National Security and our Individual Freedom." In this policy statement one of our recommendations had to do with the National Security Council, which includes the President and Vice President, the Secretaries of State and Defense, the Director for Mutual Security, and the Chairman of the National Security Resources Board. The National Security Council was created to advise the President on the integration of domestic, foreign, and military policy, and the President was authorized to appoint additional members. We then stated that the Security Council did not seem to be living up to its potential and recommended that this agency be developed as the principal executive agency on which the President could rely for formulating and reviewing comprehensive and balanced security policies. We further recommended that three full-time civilian members without other governmental responsibilities be added to the council. One of these members, we stated, should be made responsible for a more effective flow to the public of information relating to national security. It should be a major duty of all three of these members, as well as of the other members of the council, to concentrate on over-all security planning—seeing that planning ties together, in one consistent program, policies of a military nature and those concerned with nonmilitary consideration.

Without adequate knowledge of the nature of the threat to our security we leave ourselves open to any number of serious mistakes. We may acquire insufficient defenses or the wrong sort of defenses. Or we may overarm and expend such a large part of our resources on military defense that we will weaken our domestic economy, preventing us from helping other free countries to improve their conditions.

These problems are serious, but with a fully informed public and active discussion they can be intelligently met.

CANDID CAMERA

We click a moment fast in time—
The road-hill where the wagons climb.
The red dust on the highway spills,
And thistles color all the hills.
Along this curve the flowers are splashed
Where oceans of the fields are dashed.
The square of patchwork lies behind—
See it all there with what we find!—
And on one corner the steel head
Of water of the bay is spread.
Hold for an instant the land taut:
Then, presto! Look what we have caught.

DANIEL SMYTHE

Philadelphia

The Survival of Physical Science

R. B. LINDSAY

After his undergraduate work at Brown University, Dr. Lindsay went to MIT as instructor of physics, receiving the Ph.D. in theoretical physics from that institution in 1924. He was a fellow of the American-Scandinavian Foundation in Copenhagen in 1922–23, returning to this country to teach physics at Yale until 1930. In that year he went back to Brown and since 1936 he has been Hazard professor of physics and chairman of the department. He is also director of the Ultrasonics Laboratory and the Research Analysis Group, working on underwater sound.

THE theme of what follows is the survival of pure science in the modern world. In particular, for present purposes, science will be interpreted as physical science. To my colleagues in the humanities and social studies who often look with envious eyes at the booming research in physics and the other natural sciences, any concern with such a question as the survival of science probably appears supererogatory in the highest degree—even ridiculous. How can there be any question of the survival of science when scientific activity rules the roost and gets the money, especially the public money? Indeed, is not the danger quite the reverse—that in the face of increasing preoccupation with the technical details of national defense, demanding the development of more and more elaborate and intricate scientific weapons, the humanistic and liberal studies may get crowded out altogether? Many scholars genuinely fear a trend in this direction. Although their anxiety merits respect, the lack of courage and faith they display is somewhat surprising: their insight into the depths of the human soul would not appear to be as profound as is usually claimed on behalf of humane letters. As long as man persists on earth, his chief interest will continue to be *man* in all his manifestations, good and evil, understandable as well as inscrutable. Anyone who seriously believes that the insatiable curiosity about nature and the rigid mental self-discipline that are the characterizing marks of the scientist will ever manifest themselves in more than a negligible percentage of the population has little regard for the teachings of history.

This reflection aptly sets the stage for the question just raised: Can physical science survive? In the first place, what do we mean by physical science? I take it we can all agree that it is an attempt to describe a portion of human experience. Out of the totality of sense perceptions a certain realm is abstracted for special study. By observation and experiment a body of physical facts emerges, such as, for example: an iron rod when heated expands, or a body free to move in a vacuum near the earth's surface always moves toward the earth. For the more precise description of such facts, concepts, or constructs, are invented, usually expressed in terms of primitive undefined notions like those of space and time—e.g., the concept of temperature, the concept of acceleration. For greater precision and convenience of handling by standard mathematical manipulation these constructs are usually expressed in symbolic form. Next, guided by experience, but calling freely on his imagination, the physical scientist postulates certain relations among the concept symbols appropriate to a certain set of phenomena. Along with the concepts themselves these relations form the hypotheses of the physical theory intended to be the ultimate description of the phenomena. As illustrations, recall the so-called principles of mechanics, the postulates of the kinetic theory of gases, etc., etc. Physical hypotheses are commonly, though not necessarily, expressed in the form of differential equations the solution of which can be effected by mathematical manipulation. The results are relations containing symbols which must be identified with observations or operations using laboratory equipment, the construction of which is also part of the fundamental hypothetical structure of the theory. These relations are called physical laws. Upon the indicated identification they are susceptible of experimental test. If they agree with experiment the theory is said to be so far a successful description of the portion of experience to which it has been applied.

The methodological process thus set forth baldly and inadequately certainly appears to have a chilly

and forbidding arbitrariness. In the light of "Voyage to Laputa," it would be interesting to see what Dean Swift could do to it! But it has one merit, at any rate, over the extraction of sunbeams from cucumbers: It works! I do not need to enlarge here on the extent to which the ultimate technological applications based on this method have altered ways of living through the past three centuries. When we consider the enormous and ever-increasing impact the method of physical science is making on contemporary civilization, how can it possibly make sense to question its survival?

So we come back to our initial question with a spirit of skepticism. What *are* the dangers that threaten physical science? I can think of four in particular. (It should be emphasized that the setting down of these items is not done in any dogmatic or pessimistic spirit, but purely for purposes of discussion.) The dangers are, then:

1) The melancholy fact that the vast majority of living persons, the lay or general public, still labors under a profound misunderstanding of what physical science really is.

2) The ethical problem posed by science in the service of society and the state.

3) The increasing preoccupation of physical scientists with technological and specifically military applications, and the so-called programmatic research tending to submerge the individual scientist in the research team or group.

4) The relatively small number of competent persons whose coefficient of disinterest is great enough to lead them to prefer a career in pure science.

Let us examine these points. Does the fact that we are said to live in an age of science mean that the method of physical science has penetrated into the thinking habits of the average citizen? Anyone who answers this in the affirmative must be prepared to explain many curious and highly publicized phenomena of very recent date, among them "dianetics," miscellaneous "miracles," and the renewed controversy over dowsing, not to mention business as usual (and very good business, indeed) by the astrologers and the spiritualists. I hope I shall not be misunderstood at this point. There have been cranks in all ages, and in a free society there is certainly a place for them. They undoubtedly have a definite contribution to make to civilization: They amuse some persons and stimulate others to useful ideas; occasionally they make money. I am not attacking cranks as such. What seems deplorable is that a very large segment of the educated public appears unable to distinguish a crank from a scientist. In spite of our vaunted educational system, it is all too clear that to most elements of the population scientists (and here I need not restrict myself to physicists) are merely people who collect facts about all sorts of queer things and then use the facts to make all kinds of materials and gadgets. By analogy it follows (so goes the argument) that any other presumably learned person who also announces "facts" about nature and the world becomes a scientist if he can only get enough other people to believe what he has to say.

It is possible to admit that all that has just been said may be true but to question the difference it can make to the practicing scientist or to the survival of science in our society. What of it if the general public does not understand what science is? Such an attitude overlooks one very significant element of the present situation—namely, that the general public is becoming more and more the direct employer of scientists through the federal government. If the public pays the bill, then what the public thinks science is may in the course of time have a direct bearing on what scientists are permitted to do.

I think I can hear some of my colleagues at this point muttering: Probably it is, after all, just as well that the taxpaying man in the street does *not* understand the nature of physics. If he really did, would he permit scientists to exist or, at any rate, remain on the public payroll? I sometimes amuse myself by asking my fellow-scientists whether they do not consider themselves parasites on the body politic. After all, they produce nothing but ideas, and only a few manage to do that. Their indignation at the question and its imputation is a measure of their own assurance of the ultimate value of their occupation. But I submit that until more otherwise well-educated people acquire a better understanding of the method and purposes of science our position may well remain precarious and indeed grow continuously more so.

This leads directly to the second point: the *ethical* problem. No matter how disinterested an attitude the scientist may take toward his labors as merely an honest attempt to describe in consistent fashion the content of experience, no one can afford to ignore the technological consequences of scientific research. This point has been labored so often that it is needless to repeat the now-familiar antithetical arguments, which boil down to the assertion on the one hand that the scientist owes regard first, last, and all the time to the truth

alone without regard for consequences, and on the other hand to the contrary thesis that as a member of society he has a distinct measure of responsibility for the social uses of his investigations. Here we must be careful not to run off the track. I am no seer and must decline to take a stand in the controversy in question. What I am concerned with is the possible bearing of the ethical problem on the survival of science. The people who some years ago formed the society for freedom in science were clearly much disturbed over the distinct possibility that our society might destroy this freedom by directing the activity of scientists into the channels most likely to ensure the military safety and economic well-being of the state. Possibly these fears were exaggerated. At any rate nothing much has been heard of the society recently.

I do not see how anyone can question the thesis that science must remain free if it is to remain science. A good case can be made out, however, for the argument that science *can* survive as a *free* activity even if more and more scientists must work on problems suggested by agencies of the state because of their practical bearing. The principal reason for this belief is that almost every attempt to apply science to the solution of a practical problem suggests new basic problems for investigation in order to get a better understanding of the application itself. Illustrations abound: The development of the pure science field of microwave radiation from radar, and the lively interest in the very difficult problems of transmission of sound waves through a nonhomogeneous medium from sonar, come to mind at once. Danger to pure science in military and other applied research arises only if the supporting agency insists on short-range performance and refuses to countenance basic long-range investigations to be carried out concomitantly. Fortunately, there is ample evidence that both government and industry realize keenly the advantage of encouraging fundamental research that provides new knowledge and new methods of gaining it, and hence new tools for more successful applications. It is true that in time of stress—e.g., wartime—it is a bit hard to live up to this ideal, but the increasing understanding that actual survival may well depend upon the brilliant new idea, which in general comes only to scientists left to follow their own devices, provides pretty good assurance for the survival of pure science even in war. In any case, the rather gloomy pronouncements of certain eminent scientists during the second world war, that pure science is stopped by war, now seem decidedly exaggerated.

But some people believe there is another side to the picture, and a rather alarming one. It is often presented in this guise: In the halcyon days when the man on the street did not know what a physicist was and had only a vague idea even of a chemist ("a chap who stirred things up in kettles"), science could be free because no one much cared what scientists were doing. When things went wrong and a war was lost (or some similar catastrophe), the public vented its wrath on the politicians—which is as it should be. But now the public knows who scientists are: They are the creatures who made the atomic bomb and are even now concocting weapons that will make the bomb look feeble. Suppose we get into trouble in the future. What better scapegoats than the scientists, who, after all, have made possible the magnification of our troubles? So what is to prevent the populace from rising up and wiping us out? Thus runs the argument! I confess I do not find it so silly as some of my friends do. There are too many illustrations in history of similar situations. We recall the story of the brutal murder of the mathematician Hypatia by an Alexandrian mob in A.D. 415. Then there was the execution of Lavoisier by the French revolutionists, because, as the prosecutor remarked: "The Republic has no need of chemists." Gross and misleading exaggerations? Well, perhaps, but we hear today ugly rumors of what is happening to certain scientists behind the Iron Curtain. To be sure, the liquidation of these latter-day scapegoats is not done by the proletariat but merely in its name. The victims, however, are just as dead in any case!

Let us turn to less hair-raising considerations, which may in the long run have just as serious implications. In spite of the song made popular by the cyclotron-synchrotron era in physics, *Take Away your Billion Dollars*, I suspect most physical scientists are not particularly averse to receiving large financial grants for research made by government (or, to put it more accurately, the taxpayers). They need not be corrupting if scientists will only keep their heads! Of course it is true that Faraday made fundamental discoveries with simple coils of wire, chunks of metal, and acid and salt solutions, but it is not necessarily to our discredit that we desire to create physical experience on a grander scale. There are, however, dangers in this super-research that are just as ominous for the survival of genuine science as the moral perils to the politician in his superfinancial activities. The most efficient use of funds in modern large-scale research demands careful planning. To avoid utter waste this planning must be done by top-flight scien-

tists, who to this extent at least must become administrators. There are some who fear this will remove the best brains in science from actual intimate connection with research. Possibly the really fertile thinkers will continue to dream up good ideas as they rush hither and yon in planes and trains, or as they ponder the problem of where to find that key $10,000-a-year man to handle one aspect of a big program, but there are those who doubt it. There is something insidious about administration, as anyone who has had to spend time figuring on even a modest research budget knows very well. To solve this problem there may well be merit in the plan to turn over the field of administrative science to retired professors, the superannuated elder statesmen of science.

These considerations are indeed closely connected with the third item on our list—the problem of increasing specialization and the preoccupation of scientists with technological applications involving more and more teamwork. Here the fear is that the individual scientist will become submerged in the group. Let us try to be clear and candid on this point. On the one hand the fundamental ideas of science have come and always will come from individuals; from this angle science is intensely individualistic. On the other side, fundamental ideas have always been clarified, confirmed in their ultimate usefulness, and made more fertile by sharing and discussion. To this extent science has been and presumably always will be a social phenomenon. The very validity of scientific results is commonly considered to be based on this essentially social character—i.e., the criterion of the reproducibility of experimental laws by presumably independent observers. Moreover, the use of assistants in the performance of experimental investigations and in the computation of the consequences of a theory is by no means new. What is novel in the current situation in physical science is large-scale programmatic research broken down into fragments systematically assigned to subgroups, the whole being subject to supervision by an elaborate hierarchy of responsibility. The big words themselves stress how far we have come along the road from Faraday at his workbench.

The method of course reflects the enormous success of division of labor in industrial organization. It has become the standard pattern in industrial and government research laboratories and was extensively employed on even vaster scale in the military research conducted during World War II. Many authorities have praised it extravagantly as the basic reason for the success of science in the war. Others have been more skeptical and have pointed out that the research team may well be successful in the speedy prosecution of a highly specific task involving the routine application on a large scale of well-known principles, but that it is not conducive to the creation and testing of new ideas. It is suggested that team research is useful in the quick and efficient acquisition of huge quantities of data, once it is decided what data are useful for the purpose in hand. But, on the other hand, it is not always clear at the beginning of a research project just *what* data *are* going to be useful. Anyone who was engaged in NDRC work during the war can certainly recall instances in which relatively enormous amounts of data were accumulated at great expense, with the melancholy ultimate result that they were not the *right* data after all, or had been taken without sufficient forethought, so that a vital element was left out altogether.

Here, again, we must be careful not to misunderstand the nature of science. Though it is a description of a portion of experience, the judicious selection of the experience to be created is as important as the method of description. Scientists often dislike to admit the arbitrary criteria adopted for the creation of experience in the setting up of laboratory experiments and the taking of data. And, indeed, one of the commonly expressed objections of the man in the street to the physical scientist in particular is that the latter is unwilling to try to give an explanation for everything that happens (including a good many things that really do not, except in someone's imagination!). Actually, of course, it is precisely this ingenious abstraction from the totality of experience that has so far led to the success of physics. Thus, to cite a modern example, it is by no means certain that the mountains of miscellaneous data the nuclear physicists are accumulating at such a terrific rate will render the theoreticians' task any easier. The present situation with respect to elementary particles is, to say the least, confusing, and the time is ripe for a new synthesis. It is hard to see just how this is going to come out of a research team. It is far more apt to emerge from the brain of a Dirac or a Heisenberg.

It must be admitted by impartial observers that team research does lead to the efficient production of better and more elaborate instrumentation. This is beautifully illustrated by the great quantity of electronic equipment made available during and since World War II. The pure researcher will not scorn this: it has placed at his disposal research tools that have opened up vast new avenues of in-

vestigation. An excellent case in point is provided by acoustics. Although the fundamental theory of this subject may be said to have reached in most respects the form it still retains today, with the publication of Lord Rayleigh's *Theory of Sound* in 1877, its effective application to practical situations remained for years severely handicapped by the dearth of precision methods of producing and measuring sound. To acoustical experts it must forever remain a melancholy fact that even at a time when optical interferometry was introducing high precision into the experimental study of light, acoustics was still plodding along with tuning forks, whistles, resonance tubes, vibrating rods and strings, and sensitive flames. The picture changed only with the introduction of controllable electrical oscillators operating over a wide frequency range and making possible corresponding mechanical vibrations. Modern electronic instrumentation has led not merely to a host of new practical applications of acoustics but, what is more important, has turned acoustics into a valuable aid in the study of fundamental properties of matter. Ultrasonics, the science of inaudible sound, can not only age whiskey and detect submarines, it can also tell us a good deal about the molecular structure of liquids and gases.

Critics have, of course, often stressed that the use of more and more elaborate instrumentation promotes specialization, and this in turn fragmentizes even a science like physics, so that physicists are no longer just physicists but rather *solid state* physicists or *nuclear* physicists or *ion optics* physicists or *acoustical* physicists, etc., etc. The same situation prevails to greater extent in chemistry. But the grand old principle of compensation operates even here: The very existence of fragmentation in the sciences has stimulated methodologists and philosophers of science to greater attempts toward finding and emphasizing unifying features.

It is scarcely necessary to do more than recall the promising activities of the Unity of Science movement. To those who complain that this is too much tainted by logical positivism and the Vienna circle, one might reply, as a certain college president is said to have remarked to one who found distasteful a gift to the institution by a family the origin of whose wealth was socially somewhat dubious: "The only trouble with this tainted money is that 'tain't enough." Perhaps scientists can stand further contamination by the logical positivists if it only stimulates a broader point of view. In any case it is good to know that there exist other agencies also working for unity in science, among them the Foundation for Integrated Education.

So we come to the final element of danger which some consider threatens the survival of physical science. This is, of course, the relatively meager source of supply of potential scientists, and particularly physicists. Even with the stimulus of two world wars which, as far as the general public is concerned, put chemistry and physics on the map, the number of physical science majors in our colleges and universities remains relatively small; what is probably more significant, any gain in numbers has been acompanied to a large extent by a decline in quality. Let us not indulge in diatribes against our secondary school system; it is, however, a lamentable fact that the teaching of high school science is in general ineffective and the teaching of mathematics almost as bad. Without question this decreases notably the number of potential scientists on the university level and renders more difficult the task of the college teacher of physical science. We are not discovering the latent scientific talent in the youth of our country early enough and we are not giving it sufficient encouragement once it has been located. The statement of the problem is easy, but its solution is formidable, involving such far-reaching changes in our educational ideals as can scarcely be expected to take place in the foreseeable future. In the meantime we desperately do need more good scientists, if only from the standpoint of national safety.

Probably not everyone will agree with the last statement. The author of a recent book, whose ostensible aim is to deflate the pretensions of science, complains that what he calls the mass production of scientists in our universities merely produces a host of mediocre, not to say stupid, individuals who are fit only for routine jobs, which presumably could equally well be done by individuals with less pretentious and less expensive educations. I fear the statement cannot be denied, but the statistical implications seem to have been overlooked: The only safe way to ensure a large supply of able scientists is to encourage and educate as many promising specimens as we can lay our hands on!

Fortunately there are agencies that are aware of the problem and are attempting to do something about it. The Westinghouse Science Talent Search, now in its eleventh year, is well known, and, though there are those who do not feel altogether confident about some of its methods, it has been functioning long enough that one cannot help but feel some faith in its usefulness. Undoubtedly we need more such programs on a regional basis. Several of

these now exist—for example, the New England School Science Council, which operates under the sponsorship of the American Academy of Arts and Sciences. It was my privilege to have been associated with this group in its pioneer work in developing and spreading the Science Fair movement throughout New England schools. This is only one activity of the council. It is also trying, among other things, to foster in-service training of secondary school teachers of science and mathematics, and in general to promote greater cooperation between high school and college teachers of science.

After potential scientists have been caught, they must be taught, and this raises some interesting and cogent questions, particularly in the field of physics. The demand for physicists to do military research during World War II disrupted the teaching of physics in many institutions of higher education. The reorganization of departments after the war was difficult in many cases; it has now been made harder by renewed demands on top-ranking university physicists by the new mobilization. Moreover, it is easy to observe a tendency on the part of the new Ph.D.s in physics to take government or industrial research positions in preference to teaching jobs. This can have serious consequences for the future of the science. To redress the balance, the universities must seek to provide inducements, which on the financial side they are woefully unprepared to offer. It is just here that government research contracts can be of the greatest value, for they not only encourage research and the training of physicists, but they also serve to keep good teachers of physics in the universities. We here assume that the most successful teachers are those whose research ability justifies partial support of their budgets through such research contracts. This will hardly be disputed by most physicists. The various agencies of the government that award research contracts can here render the cause of science an incalculable service by helping preserve intact the efficiency and success of science teaching on its higher levels.

How shall we sum up? Four possible sources of danger to the survival of physical science have been briefly discussed. We have touched on the general misunderstanding of the nature of science prevailing among the general public, the ethics of science in the service of the state, the problem posed by increased programmatic research, and, finally, the shortage of candidates among our young people for careers in science. Well, what of these dangers: Are they real, or have we been merely flogging a dead horse? One of the propensities of scientists, and of other professional individuals, for that matter, which most men in the street find exasperating, is the habit of maintaining a judicious balance about most questions under discussion, instead of coming right out with dogmatic emphasis and saying: "*This is so*, and make no mistake about it!" Such exasperation is natural, for the judicious attitude always makes decisions difficult, and most of us have to make decisions before the evidence is all in. But our aim has been not to beat our breasts and bewail the possible twilight of physical science but rather to encourage the careful study of certain possible dangers. All scientists and all those concerned with the financial support of science should give these maters thoughtful consideration.

CONFLUENCY

Along the curves that fit the swelling hill
The climbing wood roads gently try to pull
Perspective like a trailing plaited veil.

Redbud trees and flowering dogwood fill
Converging folds, or snow's white cashmere wool,
Or autumn maple leaves beside the trail.

Then tangent to the hill, high in the sky,
They seem to find a focus till they lie
Beyond the finite law that they defy.

RACHEL GRAHAM

Clinton, New York

Cleared for Top Secret

The author is a native-born American citizen who was educated in one of our major universities and medical schools, and who is now a professor in a leading medical school. He made extensive confidential studies for the government and for our armed services, before, during, and after the war both in this country and in the theatres of active warfare. Any inquiries addressed to THE SCIENTIFIC MONTHLY *concerning his article will be forwarded to the author.*

THE first official letter had arrived just four months earlier. It said that because I was presumed to have Communist ideas and to have affiliations with Communist and Communist-front organizations, it would be "inimical to the best interests" of the country to allow me to continue the confidential military research I had been doing for years—incidentally, without compensation.

The second official letter came one week ago, just four days after a hearing in the Pentagon. It said that the earlier ruling had been reversed, and that I had been cleared "for access to all military information through top secret."

In the intervening months, I had reviewed fifteen years of my life.

I

The arrival of that first letter had been a strange, black moment. I sat at my desk for a long time, silent and unbelieving, and in deepening sadness; just as I had sat six years before, staring at a War Department telegram, which notified me that for several weeks my son had been missing in action. Here was the same desk, the same immobility, the same bleak feeling of disbelief, the same descent into unfathomable sadness. . . .

Presently I did what had to be done. This began with a call to my lawyer for guidance in preparing an appeal, which led to my finding myself some months later at one end of another table, facing the broad windows of the Pentagon and looking out over the fields and parkways that stretched away toward the Potomac. At the other end of this table was the Review Board, consisting of an Army colonel, an Air Force colonel, a Navy captain, the civilian chairman, and the executive officer. Between us on one side were my lawyers, and on the other the board's stenotypist.

It was strange to sit there waiting for the hearing to begin, heavy snowflakes swirling outside, veiling the Washington Monument and the other familiar sights of the capital. Indeed, the whole day was strange, that day of long minutes and longer hours. To me, as to many others, the Pentagon is a typical American shrine, an object of loving derision, the butt of wonderful, irreverent, debunking American humor, yet secretly an emblem of pride, hallowed by the great names of the war years. Then it had been a city in its own right, hiding the vast and intricate secrets of the war effort, filled night and day with thousands of workers, always hurrying, hurrying toward one goal. It had housed the round-the-clock labors of anonymous men and women, that nameless hierarchy of devotion and effort, working from dawn to dawn through months of anxiety to the triumphant tide of victory. . . . Then after the war I had seen the Pentagon transformed into empty halls of silence: a symbol still, but now of the transient peace of a victory we had not had the integrity or the intelligence to insure. It had dignity in its silence, but it was sad and abandoned, like an old family seat from which everyone has departed. . . . So it had been for five postwar years; but now, as had been predicted so often through those years, feet were scurrying through the halls, the cafeterias were filling up, and lengthening queues again were waiting for buses and cabs on the ramps where just a year before one would have found only an occasional lonely fare.

For me, however, it was different. During the war in a small way I had been a part of it. Now in the midst of the gathering storm, in this building where I had felt a sense of dedication, I was explaining myself. I was not "on trial" in any strictly legal sense, but on trial nonetheless, called upon to tell why my actions and beliefs did *not* mean that I should no longer be trusted with my country's safety, her defense, her secrets.

During those months before the hearing, how had I felt? Most of the time it had been so unreal that it is difficult even to remember it now that it is all over. I remember that I was calm inside. I had been confident of the outcome, yet I had felt sad all the time. I woke up sad, and I went to bed sad, almost as though I were in mourning. It had seemed unbelievable and incredible that this

could be happening to me; yet I always realized that such slips were inevitable in the chaos of the present crisis. Furthermore, in spite of my confidence that the Review Board would reverse the previous ruling, there would be sudden paradoxical jets of doubt. Suppose that it did not turn out right? Suppose the Review Board did not believe either my flat denial of the charge or my direct and circumstantial evidence, or even my own written and printed words, in which I had expressed my anti-Communist convictions?

I knew that this had happened to others, and that it could happen to me. No one is beyond the reach of human error. Each time this possibility came to mind it would start me on a fresh search for new bits of more persuasive evidence. In the meantime, these sudden doubts would lead to others: should the decision be adverse, what differences would this make in my life? Would it take away my livelihood? What activities would it terminate? From which committees and boards would I feel it my duty to resign? What of my teaching? What of my relationship to colleagues? This last question had already become a reality. There had been no newspaper publicity, but in gathering data for my defense it had been necessary to collect letters and memoranda from many friends and colleagues. Most people have a few special confidants to whom they tell all, and each confidant in turn has one or two more. Therefore, I realized that in spite of my request that they keep the matter confidential, a not-inconsiderable group of interested, doubting, sympathizing, skeptical spectators had begun from the start to assemble around this small episode, holding aloof as they wondered how it would come out. I did not resent this, but I knew that an adverse ruling could not fail to make a difference in my relationship to most of them. Furthermore, since I could not know the identities of all these observers, even after a complete vindication I could notify only those to whom I myself had initially talked or written. This I would do, but this did not mean that all the others would be reached; and since an accusation makes more exciting news than a vindication, it is more widely gossiped about and better remembered.

More deeply troubling were thoughts of my children. One of my sons was already in government service. Another was heading toward it. What would an adverse decision do to their careers? What advancement would it shut to them if the board remained unconvinced of my loyalty? What injury would it work to my son-in-law? Through the intervening four months, such doubts as these alternated with my dominant confidence. I cannot say that they disturbed my sleep or worried me for long, but they were always lurking in the background. And now all this was in the forefront of my feelings, as I sat at the end of the long table, piled high with my "evidence," looking out through the haze of swirling snow at Washington, as that long and weary day drew to a close.

II

The chairman had opened the hearing by reading a precise statement, indicating that there were no charges against me, that I was not there as an accused person, that indeed I was under no obligation to be there at all, or to answer any questions that I did not want to answer, since I had come on my own initiative. I knew this, of course. I knew that I was at the hearing only because my self-respect as a citizen made it impossible for me not to challenge the verdict of an unknown board that I was not to be trusted with my country's safety; and also because I would not endanger the rights of my children to serve their country, by allowing such a charge to stand against their father. So the chairman's quiet reading of his formal statement did not call me out of my rambling, fragmentary ruminations until the first of my seven witnesses was called. . . .

The mention of witnesses brings to mind another dilemma that had arisen during those four months. I am a scientist, but on the side I have always had an active interest in politics, government, and economics. I have read widely in these fields, and I have argued and fortunately even written a little about them. But with the scientists and students among whom I spend most of my working life I rarely allow myself to become involved in discussions of economics and politics, whether domestic or foreign. People feel intensely about their divergent views on these matters. Therefore, among those with whom I work I avoid such discussions, lest our feelings about politics disturb the harmony of a working team in a laboratory or on a teaching staff. This now made it hard to find colleagues who knew my political views and who could produce tangible evidence about them. I phoned and wrote to many, asking if they could find any letters or recall any discussions of such matters. Most replied that they could not conceive of my having Communist ideas, since this would be contrary to every aspect of my personality as they knew it, but that they did not have a scrap of concrete evidence about my views; nor could they recall our ever having discussed such issues. This forced me to turn for evidence chiefly to friends and acquain-

tances, not to mention more casual social contacts.

It soon became apparent that I had become involved in heated political arguments under either of two circumstances: either I was fighting the *status quo* boys (i.e., those who tend timidly to defend the *status quo* at all costs, fearing any change in political or economic organization); or I was fighting those who out of a mixture of fuzzy optimism and unconscious hatreds imagine that change always means improvement. Naturally, no matter how painstakingly I explained to the first group why the blind defense of the *status quo* is the very thing most likely to throw us into the hands of revolutionary Communism, these frightened souls always looked upon me—and indeed upon all liberals—as "pink." Therefore, their testimony about old arguments would hardly be helpful to me at the hearing. Nor would the testimony of the fellow-travelers from the other end help me. True, I had fought violently against their confused and sometimes unconscious Russophilia, and especially against their tolerance for the totalitarian tactics of a Russian police state from whose brutalities they themselves are protected by several thousand miles, and by the safeguards of our own political democracy. Certainly a verbatim recording of the arguments would have left no doubt as to my passionate convictions; but without such recordings the Review Board would not have trusted the testimony. Thus it was equally impossible to use the testimony of my opponents from the left and from the right, and, as I have explained, it was difficult to find scientific colleagues who had firsthand knowledge of my political beliefs. In the end, however, we succeeded in assembling about fifty letters, telegrams, and affidavits, many of them quoting from discussions, letters, and other writings of mine. These testimonials were from friends and acquaintances and colleagues, from men with whom I worked during and after the war, men who from their personal contacts with me and from the nature of the work we had done together knew where my heart lay. . . . The gathering of this ammunition required hundreds of phone calls, letters, and conversations and took a great deal of time.

III

The hearing itself proceeded smoothly.

First, out of courtesy, my witnesses were heard: thoughtfully, respectfully, and patiently.

Then my lawyer reviewed every penny which in fifteen years I had contributed to any organization that had any kind of political slant. We produced the letterheads of these organizations, with the lists of distinguished citizens who had served as their original sponsors, and their invitations and my refusals or acceptances. We produced fifteen years of check stubs, vouchers, and receipts; and we culled from my income tax returns a list of all philanthropic contributions. By the end of the day we had presented to the patient board a carefully ordered and seemingly endless array of such "Exhibits," thirty, forty, fifty, sixty of them: scraps of paper dug out of my files as documentary evidence that a ten-year-old contribution to a once-respectable organization did not mean that I had ever been a Communist, even if years later that same organization passed under the control of a group of fellow-travelers. This was the parade of the day's proceedings, leading up to the final statement of my own credo.

So much for the hearing: But how had all this come about in the first place? For several years I had been a consultant and adviser to a research agency of a branch of the armed services. The work I had done for this agency had actually been quite unimportant, especially when compared to certain other confidential military assignments. The agency as a whole, however, had undertaken tasks of steadily increasing importance and secrecy. Therefore, over the years, its entire staff had occasionally been reclassified. Each time, along with everyone else, I had had to fill out new questionnaires and had been fingerprinted again. These routine precautions were accepted as a matter of course, which had made it all the more shocking suddenly to receive that first official notice.

This notice had included full directions on how to make an appeal from the adverse ruling; but it is noteworthy that in effect a tentative decision had already been reached and that the agency had been notified not to continue to use me as a confidential adviser, and this on unknown charges by unknown accusers, on unknown evidence, and without a hearing, thus putting it up to me to prove my innocence. This, of course, runs counter to our cherished legal tradition that assumes a man's innocence until his guilt is proved. From the first, however, it seemed clear to me that it would not be easy to find a procedure which would be fairer to the individual, but which at the same time would provide equal safeguards for our national security without dangerous loss of time. Therefore, at no time during those months did I feel that I had any right to condemn the procedure until and unless I could suggest an alternative that was not only fairer but also equally safe. Toward the end of the hearing, the Review Board itself indicated its awareness of this problem, its concern about it,

and its desire to find a better solution than the present one. I must repeat, however, that it would be naïve for anyone to imagine that a fairer method will be easy to find, for reasons to be discussed.

Nevertheless, there were moments of rebellion during those four trying months, when I would ask myself: "If I were given the power to do as I pleased, how would I set up such a procedure?" Many factors entered into my consideration of this question—e.g., the fact that it is no longer possible to get rid of tyrants by the process of democratic revolution in the old Jeffersonian sense, because today revolution can succeed only when it is part of an international conspiracy, involving a treasonable relationship with another country. There is also the enormous role that secrecy has come to play, secrecy about weapons and communications, as well as plans. When wars were fought with barrel staves, secrecy was of little importance. Now, however, secrecy has become one of the most critical of all weapons in the struggle for survival: and one Fuchs can endanger every liberty that the human race has won through long centuries of bloody struggle and sacrifice.

Such considerations as these forced me to conclude that any individual who is suspect must forthwith be removed from the position in which he can do damage, even if this works an injury to him for which it may be impossible ever to compensate him fully. I had to recognize that in this twilight zone between peace and war, in spite of every effort to be fair, a number of wholly loyal civilians will be "expended" in the war against treachery, just as men of the P.T. squadrons in the Philippines were expendable in the early days of World War II. Obviously, this puts on us an equal responsibility to set up machinery for redressing such wrongs as completely and as promptly as is humanly possible. With such thoughts as these, my recurring indignation against the whole procedure would subside; and I would go back to the boring, time-consuming business of searching out the proofs of my loyalty. Indeed, much of the whole experience was nothing more dramatic than an infinitely boring chore: and if I am to enable the reader to share the experience, I must risk boring him by taking him step by step through a few more details of the preparation for the hearing.

The board had said that we could take as many days as we needed, but by five o'clock we had finished. Yet this one day's work represented four months of patient, plodding, dull, time-killing search, a search which for weeks had consumed most of my time and that of my secretaries, making impossible much of my usual load of teaching and research and writing, and leaving little time or energy for anything else. Often the thought of the mere time that all of this was taking would infuriate me, and I would be tempted to throw up the whole thing as a ridiculous and insulting indignity, and as extravagantly costly of time, energy, and money. Then would come sober second thoughts, to settle my nose firmly against the grindstone again.

Our first task was to establish the basis for the initial ruling. This took repeated letters, telephone calls, and telegrams; but after some baffling weeks of silence, we learned that in the late thirties and early forties, I had contributed to, or been a member of, certain organizations that in subsequent years were listed as subversive by the Attorney General, or by the Un-American Affairs Committee of the Congress, or by the un-American affairs committee of a remote state. (Incidentally, it is perplexing that a committee of a state should determine the judgment or action of an agency of the federal government.) We also learned that I was supposed to have had some relationship to a defunct magazine. Finally, I was said to have signed one or more letters or petitions to the President, at least one of which was on behalf of the Abraham Lincoln Brigade. These were the grounds for the adverse ruling that had been given some months earlier. By searching my files it was possible to clarify my precise relationship to all but one of them:

1) About this organization I could find out nothing. Even the name was unknown to me. There was no evidence of my having participated in its activities, or of my having contributed a penny to it. The only evidence before the board was an article from *The Daily Worker* which included my name in a list of supposed members of the organization. Since this way of padding their lists was known to be a trick of certain organizations, this item was discounted.

2) The letters or petitions to the President were of two kinds. One was a petition purporting to be a protest against "war hysteria" in 1940. We countered this by producing several letters and memoranda I had written in the thirties and early forties. These showed that I had been a violent anti-"America Firster," and had stated my belief that we ought to get into World War II long before we did. It thus became obvious that I could hardly have signed a letter that was in direct opposition to my repeatedly expressed convictions.

The other petition had to do with bringing home young Americans who had fought against Franco, who had been captured, and who long after the

Civil War was over in Spain still were held in Franco prisons and concentration camps. I could not recall having signed such a petition, and I had no record of this; but at the hearing I told the board frankly that I would sign such a petition today if it were presented to me, and that if I had ever had an opportunity to sign such a petition, I would have done so willingly, and this irrespective of the political beliefs of any of these youngsters or of any other signers. We were able to point out that it had been the policy of the American government to bring these youngsters home, that they had in fact been brought home, and that their transportation had been paid for at least in part by no less patriotic a citizen than Bernard Baruch.

3) Early in 1946 an organization had been launched by a handful of the most distinguished scientists in this country, after their return from an international scientific congress in Moscow. Many of us had responded to their appeal by a donation of $3.00. Its purpose was wholly nonpolitical; i.e., to promote an exchange of published scientific journals between Soviet and American scientists. The journals were available to Russian scientists through their own scientific libraries, and the plan was intended not to supply Russia with secrets but as a friendly gesture of international good will toward individual scientists. Because of the unwillingness of Russia to cooperate, the project had collapsed within a few months, but when the organization ceased to function the name had been taken over by a subversive organization. This seemed to make suspect anyone who had contributed.

4) In 1944 an organization of citizens was started with the avowedly nonpartisan purpose of strengthening the liberal influences in the Republican and Democratic parties with respect both to domestic and foreign policy. I had made two small annual contributions to this organization. The only meeting I had ever attended had been for the purpose of discussing how my own scientific field was consistently misrepresented in the Hollywood films of the period. Some years later, however, both the organization and the journal in which my lecture was published were listed as suspect.

5) The primary interest of the next organization was in interracial and educational problems of the South. It has always had a distinguished group of sponsors of all political complexions, among whom are a number of avowed Communists. The majority of the backers were responsible members of the community, including judges, bankers, educators from leading universities, and the like. Nevertheless, because Communists make capital of our interracial problems, they climbed on this organization's band wagon until the organization itself became suspect.

6) There was a specialty magazine, now defunct, to which in the thirties I had contributed a few dollars. Later this magazine may have become a Communist front. Before this had occurred I had made my contribution at the request of some eager young intellectuals, at a time when one was not suspicious of the loyalty of sincere young "radicals," as unhappily many have since become.

7) With regard to all these items, it was possible to show from my files that by 1946 or 1947 I had ceased contributing to any organization that had any political complexion; and that I had done this deliberately because I had decided that since I did not have time to investigate each organization fully myself, I would not support any. In certain instances, I had let my contributions lapse regretfully, because it seemed to me that they were doing good work. Nevertheless, I had felt that I should not jeopardize my freedom to work in my own scientific field by supporting political organizations in the dark.

Before 1946 I had been less guarded in this matter. This policy proved that, although I had contributed to a few organizations which had subsequently come under the influence of the Communists, I had never joined or supported an organization knowing it to be Communist; that I had attended no meetings of any of these organizations, except in the isolated instance referred to above; and that the extent of my participation had been so limited that it did not constitute evidence that I was pro-Communist in ideas, feelings, or convictions, particularly when balanced against my anti-Communist activities and the many outspoken expressions of my opposition to all police states. These I will not take time to quote here; but the relevant documents covered a period that antedated the charges, as well as later. In addition, we had the evidence of my governmental activities, much of it of a highly confidential nature, and some of it directly anti-Communist in its avowed purpose and implications; and we had governmental officials as witnesses to verify the fact that I had taken part in anti-Communist activities that were in a high degree restricted.

IV

With my statement of my own credo, and with a brief discussion of the procedure of review, the hearing came to a close. The snow had stopped.

A milky sun was showing through the overcast. We turned away from the windows, gathered up our papers, and shook hands all around to hide our masked questions, our sudden misgivings. Then we hurried through the echoing halls, in a curious anticlimactic state of mind. We were jubilant at a job we knew we had done well, resentful still at having had to do it at all, wondering whether we had succeeded in convincing the board, and wondering, too, how long it would be before we heard the verdict. Would it be a matter of weeks or months?

We had been reasonably confident of a favorable verdict. Nevertheless, when it came within a week we were pleasantly surprised at the promptness with which the board had reached its wholly favorable decision. Why, then, this review of my experience? Partly because even though it had been too easy to be typical there are lessons to be learned from it. Throughout the hearing I often wondered to myself what would have happened if I had not been sufficiently articulate about my interest in economics and politics and government to have said my say about them in letters, in print, and in private memoranda. What would have happened if I had not kept carbons of what I had written? What would have happened if I had had no files, if I had always thrown away my unofficial correspondence, if I had not been able to pay secretaries to keep my files in order and ultimately to search them for me? With a rueful smile I thought back over the years to the parody of *The Rosary* written by one of my friends to celebrate my twenty-first birthday. It ran:

> The Hours I spent with Thee, Dear Heart,
> For Fear that Memory some Day had missed 'em,
> I wrote them on a Card and placed them in
> My Filing System . . . .

Certainly in this affair "My Filing System" had paid off handsomely. But what happens to those who do not have the opportunity, the inclination, or the occasion to keep detailed personal files, and who therefore have to depend entirely on personal testimonials?

Another problem arises through the cost of such an appeal. By the time it was over, I had lost nearly four months of work, and the direct and indirect costs of the experience had run close to $3,000, even the tax deductibility of which is uncertain. What happens to those who do not have the reserves to be able to meet such a financial drain? And, finally, what could I have done if my path in life had not brought me into contact with people whose authority and prestige and significance in the community carried weight? How can a man of less financial resources and fewer contacts meet such a challenge effectively? How many such cases are allowed to go by default for these reasons alone? If it is true that at this juncture in our history considerations of national safety force us to throw men out of their jobs in so peremptory a manner and to act in effect as though these men had been judged guilty before they have had a chance even to hear their accusers, is it not essential that there should be some official provision for their protection and aftercare?

The problem is difficult both from the point of view of the country's security and from the point of view of fairness to the individual. Most will agree that the situation is so charged with danger that arbitrary action must sometimes be taken, at least to the extent of removing from the possibility of doing harm anyone who is in a position that involves the security of the nation and who is suspect. Frequently this will work an unfair and sometimes irreparable injury to individuals and their families. Against these unjust injuries certain safeguards should be taken.

First, provision should be made for adequate defense, at minimal or no expense, for the individual who is undergoing investigation, or who is appealing from a ruling such as the one described in this article. Whether this should be arranged through some type of public defender, paralleling the prosecuting attorney, or by some other device is a matter to be decided by those who are experienced in the ways of the law and government.

Second, some provision should be made to minimize or undo the harm to innocent individuals who are involved in such actions. These fall into three major groups:

a) There is a group of suspects who are cleared promptly after careful investigation. These are least injured, since they can usually resume their former work, or work closely allied to it. Yet even after they are cleared the mere fact that they have been under investigation echoes in many minds. An accusation is always more newsworthy than a clearance. Even when there has been no publicity, in his attempt to assemble data to clear himself the suspect has had to go to many people, some of them friends, some of them acquaintances. These inquiries are noised around. Consequently an individual rarely comes out of such a trial with his name untarnished, no matter how quickly and completely he has received full and formal clearance. Is there, then, any public affirmation of such a man's loyalty which would effectively lift from him the haunting shadow of the suspicion of which he has been cleared?

b) There is another group of suspects, perhaps the largest, who suffer more severely and more unfairly. These are individuals against whom nothing definite can be proved, but who cannot be completely cleared. They are a tragic company. Among them are undoubtedly some dangerous enemy agents whose identity cannot be established and whose guilt cannot be proved. These are lumped together with others who are wholly and completely loyal and innocent. Take, for instance, one mature scientist and another valued public servant. They are in their early fifties. Each in his youth had been both idealistic and radical. One had had many Communist friends. Another had briefly been a member of the Communist Party. Both had long since given up all such association, both formal and informal. In years past, however, no one gave heed to such things, and neither had taken any formal steps to sever these associations. They had simply allowed them to drop into disuse. The Party member acted as any man might have acted who had decided no longer to be a Democrat, but to become a Republican, or vice versa. He would not send a formal letter announcing to either party his change of political allegiance. In the past no one would have thought of announcing formally a change of heart about Communism. It was looked upon as a private and not a public concern. Therefore, many people who in their youth had flirted with Communism have long since turned their backs on it without taking any formal steps. Or, again, even if more recently they have taken formal steps, and even if they have joined some well-known anti-Communist organization (such as the church, for instance), it may still be impossible to know whether this has been done merely to put themselves in a position where they can do even more harm.

In this epoch of danger many individuals live in this dim twilight zone, in which no charges can be proved against them but in which they cannot be fully cleared of suspicion. Some of these, as we have said, are secretly subversive—i.e., they will be guilty, yet their guilt cannot be proved. Others are guiltless but cannot establish the fact. These last are in a situation not unlike that of many loyal Japanese-Americans during the war, against whom no un-American action or feeling could be proved, but whose origins and past allegiances were such as to make it necessary to put them in a position in which for the duration of the war they could not do harm. It must be remembered, however, that the duration of the war was a limited period, during which they were protected. They were housed and fed, and when the war was over, they were returned to liberty and to their work; and their dignity was restored to them. The cold war is of uncertain duration. The present dangers may last a lifetime. Yet we have made no provision for the innocent victims of their own youthful idealistic radicalism, who cannot be distinguished with certainty from those who are actively disloyal and subversive. We must find some way in which the innocents among them, and their families and their children, will not suffer any more than is unavoidable. That they must be regarded temporarily as in some measure civilian expendables is unhappily true, but it should be our democratic and humane duty to protect them from every unnecessary suffering. This is our duty to them, to our fine democratic traditions, and finally to ourselves and to our need to use our trained manpower wisely.

c) Over the third group, whose guilt is proved, we need waste no thought. The due process of the law will take care of their fate.

V

These, then, are some of the defects of the existing procedure even under the most favorable circumstances. On the other hand, the attitude of the Review Board as it heard my appeal was so open-minded and fair and thoughtful and attentive that after the hearing I was moved to write the following note to the chairman:

My Dear Mr. X:

After the hearing on ———, I returned to ——— deeply sensible of the friendly courtesy of your Board to my witnesses, and to my counsel, and to me. Moreover, as an American I felt proud of the consideration with which while safeguarding the public interest you also protected the reputation of one unimportant citizen; and proud too of the meticulous care which was used in uncovering and correcting a temporary injustice. I want you to know that I felt this way even before receiving the official notice of your favorable ruling.

Will you express my respectful admiration and my gratitude to the members of the Board.

Very truly yours,

It seems to me that in these days of searching self-criticism, it should not be forgotten that although we make fantastic blunders under the influence of hysterical panic, legitimate fear, political pressures, and the venal exploitation of our danger by unscrupulous politicians, nevertheless we have not forgotten to set up a democratic machinery to correct these mistakes. The blunders, their correction, and even the inadequacy of the present corrections are America as we know it. Our goal must be to minimize the former and to strengthen the latter without jeopardizing the security of all.

Administration of Research in the National Institutes of Health

W. H. SEBRELL and C. V. KIDD

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THE administration of research in the federal government has become a matter of major public importance over the past few years. Federal research and development expenditures have risen from 73 million dollars in 1940 to 840 million dollars in 1950.* A large share of these funds is expended to operate federal laboratories, and the nature of the work done in them has an ever-increasing influence on national defense, the productivity of the economy, and the health of the population. The remainder supports a wide range of research and development programs in nonfederal institutions.†

The effectiveness of federal laboratories in producing research results—applied or fundamental—is one of the major questions inherent in the growth of federally conducted research. The mission, size, organizational status, and field of endeavor of these laboratories vary so widely that few generalizations as to the most effective way to administer research in the federal government have been stated, and even fewer are generally accepted. The same seems to be true of industrial research and development, and of the rapidly expanding research and development programs of the universities.

The state of the art of research administration is such that each administrator must find his own salvation. The guidance that he can derive from the experience of others is rarely firm. For some time to come, it appears that one avenue of progress toward a body of reliable doctrine will be the accumulation of the firsthand experience of individuals and of organizations.

This article sketches some of the issues, some of the answers, and some of the unresolved difficulties faced by a large federal medical research organization—the National Institutes of Health.

The Mission and Structure of the National Institutes of Health

The National Institutes of Health is a federal agency. Within the federal government, it is the principal research arm of the Public Health Service, which is in turn a part of the Federal Security Agency. The work of the institutes falls into two general categories. The first is support of research—primarily in medical schools and universities—through grants. The second is the conduct of medical and related research in our own laboratories. The *Biologics Control Act* is administered within our laboratory research organization. Although operation of the research grant program involves a number of administrative and policy

* *First Annual Report of the National Science Foundation, 1950–1951,* p. 31. Excludes indirect costs and construction costs.

† The allocation of large sums by the federal government to private universities and other institutions raises a wide array of problems that will be resolved satisfactorily only by the exercise of the highest quality of judgment. The viewpoint of the Public Health Service on some of the basic issues relating to the research grants program which it administers is discussed by L. A. Scheele and W. H. Sebrell in "Medical Research and Medical Education" (SCIENCE, **114**, 517 [1951]).

questions, this article will deal solely with the operation of our laboratories.

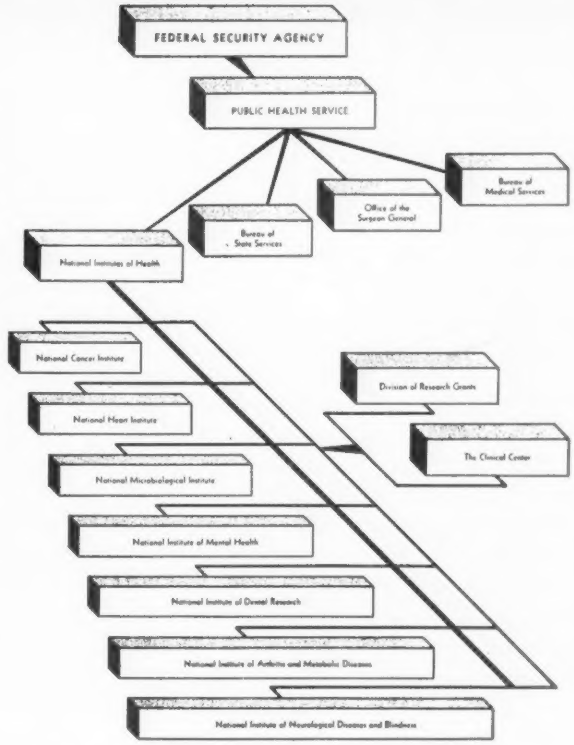
Structurally, our research is carried on in seven sets of laboratories—the National Cancer Institute, the National Microbiological Institute, the National Institute of Arthritis and Metabolic Diseases, the National Heart Institute, the National Institute of Dental Research, the National Institute of Mental Health, and the National Institute of Neurological Diseases and Blindness. Each of these is headed by a director, who is the counterpart of the director of the National Institutes of Health and, in effect, a member of our "board of directors." Each institute has a laboratory research program headed by a scientific chief, responsible to the head of his institute.

Some of the laboratory programs are small and are just being organized. Others have been operating for many years and are quite large. For example, there are 30 professionally trained employees in the National Institute of Dental Research, but there are 130 scientists in the National Microbiological Institute and 200 in the National Cancer Institute. All in all, we have about 750 employees professionally trained in a wide variety of disciplines at work in Bethesda and in a number of field stations. Of these, about 500 are scientists who plan and execute their work with varying degrees of independence. Total employment—including scientists, nonprofessional scientific personnel, and various service and administrative personnel—is about 2600.

A 500-bed Clinical Center is now under construction. In this center, it is the aspiration of our staff to link clinical and laboratory research in a way that has rarely, if ever, been achieved in medical research. Full staffing of these facilities would nearly double employment within three to five years. Total employment in 1937 was about 1000.

At the National Institutes of Health, we face a number of interlocking dilemmas. The root of all of them is the necessity to reconcile freedom with the organization of effort that is essential if bigness is not to lead to anarchy—a problem that is not unique in our society. The problems can be stated in these terms: How can a set of institutes directed by law to investigate certain diseases allow the degree of freedom of inquiry that is essential to productive research? How can a set of laboratories have a "program" and still leave investigators latitude to follow leads?

Questions of this nature are faced by most laboratories, even though the general mission of laboratories varies widely. On the one hand, a



Organization of the National Institutes of Health.

laboratory must operate so that its general function is adequately performed. On the other hand, the environment that promotes the effectiveness of individual scientists must be established. The two objectives, which often conflict, are usually reconciled, not by a clear decision, but by a process of continuous adjustment.

This discussion is in a sense a case study of the guide lines that a large set of federal medical research laboratories has developed for resolving the classic problem of adjusting the individuality of the scientist to the demands of organized effort.

The Role of Congress

Each of the institutes comprising the National Institutes of Health has a statutory definition of its mission. The National Heart Institute, for example, is directed . . . "to conduct . . . researches, investigations, experiments and demonstrations relating to the cause, prevention, and methods of diagnosis and treatment of heart disease. . . ." The task of each institute is defined in comparable terms. These statutory missions circumscribe, in a general way, the freedom of the director of the National Institutes of Health, of the laboratory director, and of the individual scientist to select his problem.

In practice, the existence of a general statutory mission is not stultifying. The area of investiga-

tion assigned to each institute is so broad that a great deal of latitude exists in the selection of lines of investigation. Research in microbiology, for example, covers a much broader spectrum of diseases, disease agents, and methods of approach than could ever be fully exploited by a single laboratory. The fields of "heart disease," "cancer," and other disease groups assigned to the various institutes are also so broad that the potential field of study within the Congressional mandate is greater than a single set of laboratories is likely to encompass. Moreover, as a matter of policy, research in each institute is not confined to studies that are precisely and narrowly identified with the group of disorders falling within the province of the institute. Our philosophy is that an understanding of the basic disease processes will be impeded unless our investigators have elbow room to explore the fundamental relationships among body systems and the fundamental biochemistry and biophysics of protoplasm.

The Congress has understood these policies, and the general Congressional mandate has not in practice prevented us from conducting a research program that provides what we believe to be the optimum degree of freedom of the individual. As a case in point, the Congress in 1949 refused to enact bills providing for a large number of specialized institutes—for example, for the study of multiple sclerosis, diabetes, poliomyelitis, and epilepsy. Instead, two institutes covering the broad range of metabolic and neurological disorders were set up by law.

At the same time, Congress has, by establishing institutes for the diseases that can be generally defined as chronic or degenerative—such as heart diseases, cancer, and the neurological and metabolic diseases—altered the general emphasis of research toward the diseases that are increasingly important causes of illness, disability, and death. Thus, the general content of our research program is strongly influenced by public opinion, as reflected through Congress. This influence is not only inevitable but desirable. Before 1937, the National Institutes of Health was concerned quite heavily with communicable disease problems. Since then, the rate of expansion in other areas has been more rapid than in microbiology. As a result, only about 20 per cent of our research budget is now devoted to studies in microbiology. The remainder is for basic research not specifically applicable to any one disease, or for problems suggested by the chronic diseases. In view of the dramatic shift in causes of illness and death away from communicable diseases and toward chronic and degenerative diseases, this change in our program is quite rational. In

New Clinical Center of the National Institutes of Health, which is nearing completion. View of east side.

In a study on obesity being conducted at the National Institute of Arthritis and Metabolic Diseases, the mouse at the right was injected with a fat-producing substance. The one at the left is of normal size.

all frankness, however, the Public Health Service alone could probably not have convinced Congress that such a change was called for. The most important cause of the shift was massive public demand reflected in Congressional action.

Some Congressional leaders have a real understanding of and sympathy for medical research and of the conditions under which research will prosper. These men have realized how the nation depends upon advances in knowledge as a means of ultimately raising health levels and of reducing the economic burden of medical care. They have argued the case for medical research vigorously and intelligently.

The Role of the Director of the National Institutes of Health

Congress not only sets the broad mission of our laboratories in substantive, permanent legislation, but appropriates funds annually to operate them. Each institute and the National Institutes of Health as a whole have separate appropriations. Thus, not only what we will do—in general terms—but how much we can do—in very precise terms—is set by Congress.

These decisions as to what and how much are transmitted through the administrator of the Federal Security Agency and the Surgeon General of the Public Health Service to the National Institutes of Health. As is true of Congress, both the administrator and the Surgeon General influence the substantive research program only in the most general terms. They are concerned with the proper degree of integration of the work of the institutes with other research and general public health programs of the Public Health Service, and with other national activities. For example, such questions as the role of the Public Health Service in defense against armed attack, the place of the Public Health Service in determination of the toxicity of various substances, and the relative role of research as compared with other public health activities must be dealt with by the administrator and the Surgeon General.

As decisions as to what and how much are transmitted to the director of the National Institutes of Health, their effect upon administration merges. The scientific director of an institute must know in general terms not only the limits of the area of investigation open to his staff, but also the resources available to him. The process by which decisions on these matters are arrived at in a large research organization is subtle and complicated. Such important matters as the nature of the relationship between the front office and institute scientific directors, the character of informal channels of

communication, and the process by which the ideas and accomplishments of individual investigators are translated into research projects and into budgets cannot be described briefly without gross oversimplification. At the risk of such oversimplification, however, some of our major ways of operating, beginning with the director's office, can be outlined.

The director of the National Institutes of Health and his immediate staff deal with few matters of scientific substance. We try to set the spirit, goals, and objectives of the organization. We fight battles for the organization as a whole. We assess, and determine how to adjust to, pressures of various kinds that are brought to bear upon the organization. We attempt to anticipate areas of conflict among the various institutes and to resolve conflicts if they arise. We try to set the most productive administrative structure. A good deal of time is spent in establishing the attitudes and machinery that will make communication effective. We do our best to provide money, men, and materials adequately and promptly. Many of the activities of the director's office are related to our Research Grant Program, an area of administration that is as significant and complex as the management of the laboratories.

The effort that an administrator of a large federal research laboratory must devote to duties arising out of his ex officio status is a major part of his job, and only rarely can these tasks be delegated. If his organization is a part of a large governmental department, as is the National Institutes of Health, he must serve on the general staff of the department both to represent his organization and to advise on nonresearch matters. He is asked to serve ex officio on governmental and nongovernmental boards and committees. He has some ceremonial duties to perform. He welcomes many visitors. He makes speeches fairly often. Activities such as these are not a useless appendage but an integral part of the work of a public servant, even though someone trained as an investigator has a natural longing for the laboratory and for scientific problems.

The director's office plays a role in the determination of the total scale of the research program of each institute, in the selection of broad areas of investigation, and in influencing the general direction of research in the institutes. The degree to which the director's office influences research programs in the institutes varies from institute to institute and from time to time. Questions of this sort are discussed with the appropriate staff of the institutes concerned: To what extent and in what manner should the research program be reoriented toward work directly relevant to national defense? How should space in the new Clinical Center be distributed among the institutes? To what extent and in what direction should existing research be reoriented to take account of promising new developments—as, for example, the startling chain of leads springing from the clinical effects of ACTH and cortisone?

In general, however, when the director's office deals with the planning and execution of individual studies, the initiative usually comes from the laboratory. The relationship between the director's office and each institute on matters relating to the substance of research is consultative and not authoritarian. We get into trouble when we lose sight of the fact that research productivity is determined primarily in the laboratory and in the minds of individuals. Unless the research administrator keeps this constantly in mind, he is apt to find himself captain of a ship that is carrying no cargo.

This philosophy differs somewhat from that which properly governs the administration of most governmental operations. Those whose background is in public administration rather than research sometimes find it difficult to grasp how a research organization can operate effectively when most of the decisions that ultimately determine the productivity of the organization are made by people who have little or no administrative responsibility.

The Role of Institute Research Chiefs and Laboratory Chiefs

The broad scientific area outlined for the laboratory, and funds and space on hand or foreseeable, set the general goals and program of the laboratory and the scope of its operations. Within this general pattern the determination of the actual substantive content of the research program is in the hands of the research chief and the laboratory heads in each institute. They decide how the general charter is to be interpreted and exploited and how resources are to be deployed. The outcome of their decisions constitutes the actual working program of the laboratory. One of the major tasks of the research administrator is to decide in what detail the program of a laboratory should be set from above. This is more an artistic than a scientific performance. Within the limitations set for the laboratory, the backgrounds of laboratory directors will lead them to select some problems for investigation and to ignore or play down others. The general state of knowledge in the field, and par-

ticularly the interests and competence of the staff on hand or available, will affect their decisions.

If a new laboratory program is being set up, the research chief can lay out a general plan of attack, recruit the people who can fit into his general scheme of research, and if necessary modify plans to fit the people available. We would rather have substantial leeway in research planning to accommodate high-grade talent than to adhere rigidly to a fixed program executed by mediocrities.

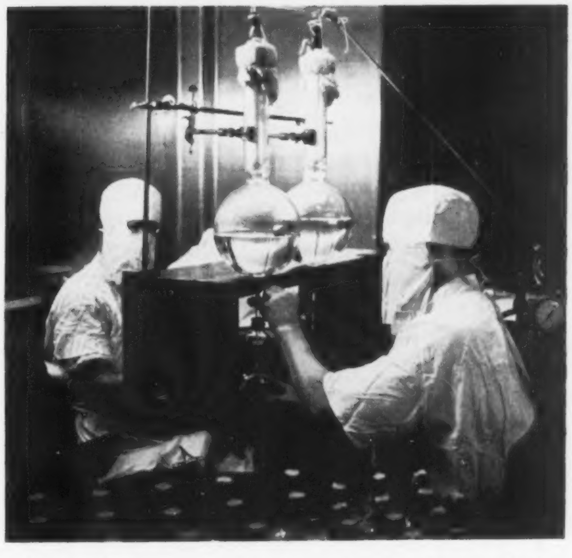
This recruitment process seems to us to be the primary means by which a predetermined general research objective can be carried out without forcing investigators to pursue lines of work that do not particularly interest them. Only rarely will a highly trained and experienced man with interests and a reputation in a specific field shift his area of interest rapidly or drastically.

After a laboratory has operated at a stable level for a few years, we find that the laboratory director has greater difficulty in changing the general work program of the laboratory. The ultimate degree to which a going research program can be changed is limited by the training, experience, and competence of investigators. Theoretically, a group of virologists can turn their attention from one group of viruses to another, or from metabolic studies to investigations relating to identification or diagnosis. But not even theoretically could they be expected to shift their studies to steroid chemistry. These limits are inherent in scientists and not in governmental operations. Within these limits, we have noticed that the extent to which research programs can be changed without changing the staff depends a great deal upon the degree to which the interests of investigators can be changed. This in turn depends heavily upon the personality, imagination, and scientific competence of the research chief.

The work of a laboratory can be shifted fundamentally and suddenly only by hiring new staff or replacing staff. This point is sometimes not realized by those who assume that either a research director or a laboratory chief can simply order a new line of investigation to be undertaken.

The Degree of Freedom of the Individual Scientist

Just as the scientific director must decide how broad the charter and how extensive the resources of a laboratory are to be, the laboratory director must decide how broad a charter he will give to individual scientists and how he will allocate among individuals and groups the resources made available to him. The laboratory director controls the



National Cancer Institute scientists filtering a solution used in tissue culture work. The solution is sterilized by filtration under pressure, then sealed in flasks for future use. A technique has been worked out for growing cells on a sheet of perforated cellophane, which affords much larger cultures for laboratory study than have ever been grown before.

money, men, space, supplies, and equipment made available to individuals. He has a fixed annual budget, just as an industrial concern has a fixed budget. He has to allocate resources in essentially the same manner as does an industrial laboratory. In this sense, our scientists are not free souls. But no investigator in industry or university has an unlimited budget. Realistically, freedom of research rests not upon freedom to requisition unlimited resources, but upon freedom to follow leads and hunches.

At the National Institutes of Health, our policy is to provide the degree of freedom to plan and conduct research that is consistent with the scientist's training, competence, and experience. The person who has just received a Ph.D. degree usually works under fairly close supervision. If he develops well, he is gradually given wider latitude to select for himself the problem that he will attack, and to design and execute his work. A substantial number of mature investigators have a competence that has earned for them a great degree of independence.

As an example, one of the investigators in the National Institute of Arthritis and Metabolic Diseases is an internationally known expert on the electron microsope. He has developed new metallic shadowing techniques and made important substantive observations relating to viruses. Although he is on the staff of the Laboratory of Physical

Biology, he plans his own work and directs a sizable staff. There is no one in Bethesda or elsewhere who could plan it for him. Our objective is to create an environment in which, regardless of the size of the organization, every investigator who has the necessary spark can achieve a comparable degree of independence. This approach, in our view, is the most effective means of counteracting the tendency of bigness to stifle individuality.

In short, we view scientific freedom—as applied to the work of an individual investigator—as a relative and not an absolute matter, and as an earned right and not as a right inherent in his status as a scientist. We try to expand the degree of freedom of our staff as rapidly as possible because we are convinced that this is the way in which basic research—our primary function—will advance most rapidly and fruitfully.

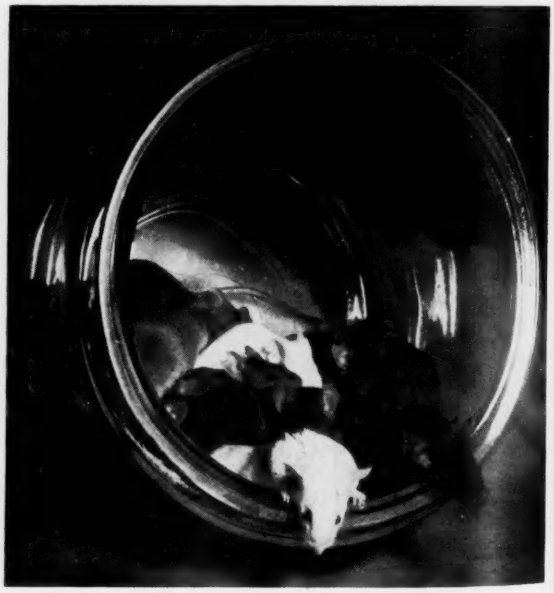
In particular, we believe that this policy is important to us as a large research organization that is still growing. As a laboratory—or a set of laboratories—grows, each individual becomes a smaller part of the whole. Even though he may be working in a relatively small group, as an individual he is a part of a larger organization. Recognition of the drive that animates all good research men is, in our opinion, a major means of sustaining the originality, curiosity, and energy of individuals in an environment where the individual is only one of many.

There seems to be an increasing tendency on the part of industrial concerns—pharmaceutical and other—to give their research staffs a wider degree of latitude to pursue basic studies not directly and demonstrably related to the business of the firm. We feel sure that our own policy of expanding the degree of freedom of our people as rapidly as they can earn it is one of the factors that enables us to recruit competent people in the face of higher salary scales in industry. As a corollary, we believe that sound research cannot be done with fear as the motivating force. Pressure to produce in research defeats itself. There is, on the other hand, no discipline so strong as the self-imposed drive that animates a good research man. We try to help the laboratory chiefs establish the conditions under which this sort of drive is cultivated and respected, rather than push for research production as such.

A policy of balancing the degree of freedom accorded to investigators against their rate of development brings sharply into focus the problem of assessing the productivity of scientists and the relative scientific value of their output. This is a very puzzling problem. At the same time, it is perhaps the most significant judgment required at any level of research supervision. It bears upon the status and promotion of individual scientists. It is the critical point to be weighed in deciding how resources are to be allocated to people and to working groups. A series of poor judgments can seriously affect the worth of a laboratory.

These judgments must in some cases relate not only to the scientific competence of the scientist, but to his personality and his way of working. This is true particularly if he has some degree of responsibility for the work of others. As an example, some senior investigators are by training and temperament inclined to set the problems to be studied by their subordinates, to plan the experimental approach, and to supervise the work of subordinates in detail. If the scientist who thinks and works in this way is really good, he will have a productive laboratory. We have outstanding investigators of this sort at the National Institutes of Health. The difficulty is that these men rarely train and keep people who can assume responsibility for a research program. Younger men of promise tend to stay as long as they can learn from the master, and then leave. As a result, the research program falls apart when the senior man departs. An institution that depends too heavily upon such men runs the danger of losing effective leadership and continuity.

We have no magic formula for arriving at our decisions. Since they rest essentially upon judg-



The role of heredity is studied at the National Cancer Institute. The grandfather and grandmother of this litter represented pure inbred strains. Note mixed colors of third generation.

ments of people, immediate supervisors are asked to appraise the people carefully. The recognition that their scientific peers give them is noticed. The reactions of people outside the organization to their development is assessed. Finally, an administrative decision must be made, often on the basis of conflicting advice. This is obviously not an objective procedure, but no rating scale or any other similar device has proved as satisfactory to our staff as the sort of careful assessment by carefully selected and competent people that they try to secure. The current efforts of a number of groups to put assessment of the competence of research workers on a more scientific basis should prove helpful.

We consider this question to be of such importance to the vigor and productivity of all laboratories that all promotions of scientists to the higher grades are approved by the director of the National Institutes of Health after recommendations have been made by a Promotion Board composed of mature scientists from all institutes. This is one area in which we have not found it possible to follow the principle of leaving operating decisions to laboratory chiefs.

Communications

At every point in research administration one encounters the problem of communications in a more or less acute form. Individual scientists must know what their colleagues are doing. The laboratory chief has to know what is going on in his organization. The research directors must know, in general terms, and sometimes in precise terms, what laboratory plans and progress are and what problems of a semi- or nonscientific nature are on the minds of the laboratory staff. Individual scientists, on the other hand, must be informed not only of general decisions that affect them, but also why the decisions are made.

Patterns of research organization may well have an important effect upon the communications problem. Some companies draw scientists with required talents together in relatively small groups for specific tasks, and then regroup them as the jobs change. This way of operating may make it less difficult to sustain the necessary degree of scientific collaboration. At the National Institutes of Health, however, there is a large group of investigators. Many of them have a wide degree of latitude in framing their research plans. In our situation, it seems best to work out with each investigator a place in a relatively permanent laboratory organization. The three elements of size, freedom, and laboratory organization combine to produce communications problems. As our organization grows we work constantly to prevent sheer size from stifling the desire of individuals to keep in adequate contact with one another.

The laboratory staffs use a variety of devices to promote the necessary degree of communication. They have guest lectures, as well as seminars on work in progress, to which people from other laboratories are invited. As another example, a series of seminars on instrumentation is being started. The precision and range of use of scientific apparatus are advancing so rapidly that men in a specialized field have difficulty in keeping up with all the equipment that may be relevant to their work. Such seminars are not set up on organizational lines, thus promoting consultation among laboratory staffs.

This horizontal communication problem is not confined to scientists at the bench. It exists also in the director's office. We resolve the problem by a session every morning for about half an hour to exchange information, discuss problems, and work out general strategy. This meeting is with the director's immediate staff of six. These men have general areas of staff responsibility, but each feels free to participate in formulating decisions that are not in his area. Each of them, moreover, is expected to, and does, work on any problem for which he seems best fitted.

Communication from the bottom up and communication from the top down are of equal importance. Matters of general concern to all institutes are brought before an advisory board composed of institute directors and the director's immediate staff. We have also established regular channels for group discussion with scientific chiefs, with the operators of grant programs, and with the administrative officers of each institute. A wide array of problems not of general interest to these groups, of course, is resolved by individual dealing between the director's office and the institute concerned.

Each laboratory chief has a similar concern for keeping channels of communication open. If communication in the laboratory breaks down, the consequences are often plain. We have found, for example, that if a laboratory chief does not take the time to explain to his people why general decisions have been taken—why budgets have been curtailed or why certain lines of work have been expanded—a morale problem directly affecting productivity is created.

Effective horizontal and vertical communication is not assured by mechanics and devices, but by establishment of the will and the desire to com-

municate up and down. This begins at the top. The research director, by his attitude and his interests, and by the questions he raises, can do a great deal to promote communication throughout the laboratories.

Despite our efforts, we do not feel that we have the answers to effective communication. One practical problem is to compress the time required for adequate communication so that too many people do not spend too much time talking and too little time working.

Business Management and Logistics

Apart from these general problems, the day-to-day problems of business management and logistics of a large research organization are acute. In our experience, this area of administration is critically important, particularly since the need for funds, equipment, and supplies required to keep each independent investigator at work is rising steadily. Each specific problem may appear to be minor, but if these details are not well handled, research will suffer and costs will rise.

Most of our ancillary laboratory services are centralized. For example, we have a central animal breeding colony, although some laboratories must have colonies for their own genetic studies and similar projects. We have a central personnel office that services all the institutes. Our library, translating, photography, and medical arts work are centralized, but, as those who deal with research people know, it is neither possible nor desirable to be arbitrary about the journals, books, or cameras that can be kept in laboratories. Our instrument and glass-blowing shops are centralized, but certain biophysicists need to work in their own small instrument shops. The purchase and supply operation is centralized, but here again there is a fair degree of latitude for direct emergency purchases by individual scientists. All the centrally performed functions are financed by transfers from each institute to a central fund. This fund is determined at the start of each year on the basis of formulas that are agreed upon as a fair measure of demand. These formulas provide an equitable and sufficiently precise measure of goods and services supplied to each institute, without an expensive cost accounting system.

Although many of our technical and laboratory services are centralized, there remains a heavy burden of administration within each institute. They have to requisition supplies and equipment. They must initiate their own budgets. Each institute has to take care of the day-to-day paper work and related problems arising at the bench that are not scientific in nature. Each institute recruits a large portion of its staff, particularly for senior positions. Only persons recruited for a limited number of key positions must be agreed upon by the "board of directors."

The nonscientific adminstrative aspects of National Institutes of Health operations are handled in quite a different manner from the actions and decisions relating to the substance of research. In general, the administrative tasks done for research are, as compared with the substantive planning and execution of research, subject to closer central scrutiny and are conducted to a greater extent under uniform patterns and regulations.

We are not sure, however, that we have struck the most efficient and economical balance between centralized and decentralized provision of services. In each of these areas, there has to be careful weighing of the gains to be derived in terms of speed and simplicity of operation from decentralized operations against the economy of central operation; the reduction in cost and increase in flexibility achieved through standardization of equipment must be assessed against the advantages of giving scientists the kind of equipment that they consider best.

We try to relieve the scientific staff of these administrative duties by using people trained in business management and public administration. Although we are sure that this policy is fundamentally sound, problems arise. Some scientists, by training or temperament, are not able to use good administrative assistance when it is available. There is sometimes a feeling among the scientific staff that the administrative people pre-empt decisions that scientists should make, that supporting services absorb funds that should be devoted directly to research, or that the services are not provided efficiently. These criticisms are a useful spur to efficiency and economy. However, we would never advocate requiring scientists to carry a heavy load of administrative detail on the ground that problems arise when others try to help them.

Some Research Administration Problems Peculiar to Government

Apart from those matters that are of common concern to research administrators in industry, universities, and government, some problems are either unique to government, or appear to be particularly important in government. Federal salary scales are not high. In general, most surveys show that federal pay scales are roughly comparable with industrial scales in the lower and middle brackets. Outstanding people, however, are paid much less

than they could earn in private industry. Our upper limit is $15,000 per year, and it took a special act of Congress to go that high for a limited number of outstanding people. The obvious question is, Why are good people attracted to government service in the first place, and why do they stay?

So far as the National Institutes of Health is concerned, we do attract outstanding people, and our turnover in the upper brackets is no more than we want to ensure a desirable infusion of new people and fresh ideas. We trace this situation, first, to our general policy of freedom of research. Second, we offer our people good equipment, adequate supplies, and adequate assistance. Third, we have *esprit de corps*. We are far from self-satisfied on this score, but unless there were at least reasonable satisfaction with the way the organization operates or an active interest in making it operate better, many more people would leave for higher paid jobs in industry.

A second feature of federal employment that affects scientists is the federal merit system. We do have a great deal of red tape to go through, but civil service regulations are not a key administrative problem. We recruit most of our scientific staff by the personal efforts of our investigators. The Civil Service Commission has given us a great deal of latitude in this respect. We have also made substantial progress, in cooperation with other federal research laboratories, in convincing the Civil Service Commission staff that research accomplishment alone, rather than the size of a man's staff or his formal place in the organization, should be the basis for setting the compensation of scientists.

We want to be able to pay adequately those scientists with heavy adminstrative duties. We must, however, be able to reward research scientists solely for their contributions as scientists. We must not be forced to reward outstanding investigators by putting them in administrative positions where they cannot do research. This sounds like a self-evident proposition, but the slowness with which this principle has been adopted is surprising—and the battle is not yet completely won. In summary, regulations sometimes hamper us, but they are not among our really significant administrative problems. They probably are no more troublesome than the problems created by the personnel regulations and practices in a large industrial concern.

On the other hand, the process of securing an appropriation is extremely complicated and time-consuming. After the National Institutes of Health has prepared a firm budget, our proposals are reviewed by the Public Health Service and the Federal Security Agency before we discuss them twice with the Bureau of the Budget—the group responsible for framing the budget which the President sends to Congress. Then we appear before committees of both houses of Congress. At any one time, we are operating on one budget, in the process of justifying the budget for a year ahead, and preparing a budget for the year ahead of that.

Mouse and "window." Studies with a transparent chamber in the mouse, a technique developed by G. H. Algire at the National Cancer Institute, have already produced several major findings with respect to tumors growing within the window.

We make a determined effort to bring scientists at the bench into this budget process only once each year and then only to the extent required for them to outline their needs. Our internal budget-making procedure, however, is still imperfect.

The expansion of medical research in governmental, university, and industrial laboratories in all probability is not temporary, but a permanent and sorely needed increase in the magnitude of research in this country. Not only is the total effort increasing, but many individual research institutions are expanding. The medical research structure of the country is adapting to this growth through the development of new patterns of organization and administration. These patterns are far from set, and many difficulties associated with research conducted on a large scale are not resolved. In this period of rapid change, communication among those responsible for research administration in universities, government, and industry is essential to the development of patterns that will best promote the total scientific effort of the country. Without conscious effort to resolve the very real dilemmas associated with growth and size, the nation will receive less than the maximum possible return for the resources devoted to medical research.

Some Issues in the Logic of Historical Analysis*

ERNEST NAGEL

The author's principal interest after receiving his Ph.D. (1931) at Columbia has been in the philosophy of science, as evidenced by his many publications in the field, as well as his being past president of the Association for Symbolic Logic, past vice president of the American Philosophical Association (Eastern Division), present vice president of the Institute for the Unity of Science, and co-editor of The Journal of Philosophy. *He has been professor of philosophy at Columbia since 1946 and has been in the department since 1930.*

ACCORDING to Aristotle, poetry, like theoretical science, is "more philosophic and of graver import" than history, for the former is concerned with the pervasive and universal, and the latter is addressed to the special and the singular. Aristotle's remark is a possible historical source of a widely held current distinction between two allegedly different types of sciences: the nomothetic, which seek to establish abstract general laws for indefinitely repeatable processes; and the ideographic, which aim to understand the unique and nonrecurrent. It is often maintained that the natural sciences are nomothetic, whereas history (in the sense of an account of events) is ideographic; and it is claimed in consequence that the logic and conceptual structure of historical explanations are fundamentally different from those of the natural sciences. It is my aim here to examine this and related issues in the logic of historical analysis.

I

Even a cursory examination of treatises in theoretical natural science and of books on history reveals the prima facie difference between them, that by and large the statements of the former are general in form, and contain few if any references to specific objects, places, and times, whereas the statements of the latter are almost without exception singular and replete with proper names, dates, and geographic specifications. To this extent, at least, the alleged contrast between the natural sciences as nomothetic and history as ideographic appears to be well founded.

It would, however, be a gross error to conclude that singular statements play no role in the theoretical sciences or that historical inquiry makes no use of universal ones. No conclusions concerning the actual character of specific things and processes can be derived from general statements alone; and theories and laws must be supplemented by initial or boundary conditions when the natural sciences attempt to explain any particular occurrence. Nor does the familiar and often useful distinction between "pure" and "applied" natural science impair the relevance of this point. For, clearly, even the pure natural sciences can assert their general statements as empirically warranted only on the basis of concrete factual evidence, and therefore only by establishing and using a variety of singular statements. And there are branches of natural science, such as geophysics and animal ecology, that are concerned with the spatiotemporal distribution and development of individual systems. It follows, in short, that neither the natural sciences taken as a whole nor their purely theoretical subdivisions can be regarded as being exclusively nomothetic.

Neither can historical study dispense with at least a tacit acceptance of universal statements of the kind occurring in the natural sciences. Thus, although the historian may be concerned with the nonrecurrent and the unique, he selects and abstracts from the concrete occurrences he studies, and his discourse about what is individual and singular requires the use of common names and general descriptive terms. Such characterizations

* Based on the author's address as retiring vice president of Section L (History and Philosophy of Science) before the Philadelphia meeting of the AAAS last December.

are associated with the recognition of various kinds or types of things and occurrences, and therefore with the implicit acknowledgment of numerous empirical regularities. Again, one phase of a historian's task is to establish the authenticity of documents and other remains from the past, the precise meaning of recorded assertions, and the reliability of testimony concerning past events. For the effective execution of this task of external and internal criticism, the historian must be armed with a wide assortment of general laws, borrowed from one or the other of the natural and social sciences. And, since historians usually aim to be more than mere chroniclers of the past, and attempt to understand and explain recorded actions in terms of their causes and consequences, they must obviously assume supposedly well-established laws of causal dependence. In brief, history is not a purely ideographic discipline.

Nonetheless, there is an important asymmetry between theoretical and historical sciences. A theoretical science like physics seeks to establish both general and singular statements, and in the process of doing so physicists will employ previously established statements of both types. Historians, on the other hand, aim to assert warranted singular statements about the occurrence and interrelations of specific actions; and though this task can be achieved only by assuming and using general laws, historians do not regard it as part of their task to *establish* such laws. The distinction between history and theoretical science is thus somewhat analogous to the difference between medical diagnosis and physiology, or between geology and physics. A geologist seeks to ascertain, for example, the sequential order of geologic formations, and he is able to do so by applying various physical laws to the materials he encounters; it is not the geologist's task, qua geologist, to establish the laws of mechanics or of radioactive disintegration that he may employ.

The fact that historical research is concerned with the singular, and seeks to ascertain the causal dependencies between specific occurrences, does not warrant the widespread contention that there is a radical difference between the logical structure of explanations in the historical and the generalizing sciences. I shall consider only one specific argument to support the claim that there is such a difference. It has been said that there is a demonstrable *formal* difference between the "general concepts" of the theoretical sciences and the "individual concepts" assumed to be the goals of historical inquiry. Concepts of the first kind are alleged to conform to the familiar logical principle of the inverse variation of the extension and intension of terms: when a set of general terms is arranged in order of their increasing extensions, their intensions decrease. But quite the reverse is said to be the case for the individual concepts of historical explanations, since the more inclusive the "scope" of such a concept, the richer and fuller is its "meaning." Thus, the term "French Enlightenment" is claimed to have not only a more inclusive scope than the term "the life of Voltaire," but also to possess a fuller intension.[1]

But this is simply a confusion, derived in part from a failure to distinguish the relation of *inclusion* between the extensions of terms, from some form of *whole-part* relation between an instance of a term and a component of that instance. Thus, the French Enlightenment may be said to "contain" as one of its "components" the life of Voltaire; and it is doubtless correct to maintain that the term "French Enlightenment" is "richer in meaning or content" than the term "the life of Voltaire." But the *extension* of the term "French Enlightenment" does *not* include the *extension* of the term "the life of Voltaire," so that the logical principle under discussion cannot be significantly applied to these terms.

More generally, there appears to be no good reason for claiming that the general pattern of explanations in historical inquiry, or the logical structure of the conceptual tools employed in it, differs from those encountered in the generalizing and the natural sciences. The explanatory premises in history, as in the natural sciences, include a number of implicitly assumed laws, as well as many explicitly (though usually incompletely) formulated singular statements of initial conditions. The tacitly assumed laws may be of various kinds. They may be statements of regularities well attested in some special science, or they may be uncodified assumptions taken from common experience; they may be universal statements of invariable concomitance, or they may be statistical in form; they may assert a uniformity in temporal sequence, or they may assert some relation of coexistent dependence. The singular statements of initial conditions are of comparable variety, and although the truth of many of them is often incontrovertible it is frequently highly conjectural. Indeed, the relevance of such singular statements to the specific problems under investigation, as well as their truth, are questions upon which historians are often undecided or unable to achieve unanimity. There are, in fact, several problems in this connection that are of much concern to historical research, although they are not without rele-

vance to other branches of social science as well. I therefore turn to consider briefly some of the real and alleged difficulties that plague the pursuit of historical knowledge.

II

It is a platitude that research in history as in other areas of science selects and abstracts from the concrete occurrences studied, and that however detailed a historical discourse may be it is never an exhaustive account of what actually happened. Curiously enough, it is the very selectivity of history that generates many of the broader questions relating to the nature of historical inquiry and is sometimes made the occasion for wholesale skepticism concerning the possibility of "objective" explanations in historical matters. Since a historian exercises selection in choosing problems for study, and also in his proposed solutions to them, it will be convenient to examine some of the relevant issues under these two heads.

1) Historians do not all concern themselves with the same things, and there are undoubtedly many past events that have received attention from no historian. Why does one historian occupy himself with ancient Greece, another with modern Germany, still another with the development of legal institutions in the American colonies, a fourth with the evolution of mathematical notation, and so on? Is there some general feature which differentiates those occurrences that are of concern to historians from those that are not? And, above all, is a historian prevented from giving a warranted or objective account of things because of his initial choice of a limited problem?

It is clear that there is no uniform answer to the first of these queries, for in historical inquiry as in other branches of science a variety of circumstances may determine what problems are to be investigated. It may be individual preference and endowment, controlled by education and the influence of teachers; it may be professional obligation or the desire for financial gain; it may be national pride, social pressure, or a sense of political mission. Historians of ideas have given some attention to this matter, and have uncovered interesting data concerning stimuli to specific investigations. But there is no prima facie reason to believe that, because a historical inquiry begins with a specific problem, or because there are causal determinants for his choice, a historian is in principle precluded—any more than is a natural scientist—from rendering an adequate account of the subjects he is investigating.

Many writers maintain, however, that the selectivity of history is peculiar in that the historian is inescapably concerned with "value-impregnated" subject matter. Thus, according to one influential view, an individual or process can be properly labeled as "historical" only if it is "irreplaceable," either because it uniquely embodies some universally accepted cultural value or because it is instrumental to the actualization of such a value. In consequence, the supposition that historical inquiry can ignore theoretical value relations is said by some writers to involve a self-deception,[2] whereas other commentators have concluded that unlike the physical sciences "history is violently personal," since "stars and molecules have no loves and hates, while men do."[3] There is, however, no basis for the claim that historical study is addressed exclusively to value-impregnated occurrences, unless indeed the word "history" is arbitrarily redefined so as to conform with the claim. For, although undoubtedly much historical inquiry is concerned with events that may be so characterized, there are also many investigations commonly called "historical" that are not of this nature—for example, inquiries into the development of the stars, biological species, and much else. More generally, there appears to be no warrant for any of the various claims that the occurrences studied by historians are distinguished by some inherent differentiating feature from those that are not. Moreover, even when a historian is concerned with admittedly value-impregnated subject matter or with occurrences manifesting various passions, it by no means follows that he must himself share or judge those values or passions. It is an obvious blunder to suppose that only a fat cowherd can drive fat kine. It is an equally crude error to maintain that one cannot inquire into the conditions and consequences of values and evaluations without necessarily engaging in moral or aesthetic value judgments.

There is also the broad question whether historical inquiry is inevitably guilty of distorting the facts because it is addressed to limited problems and is concerned only with certain selected materials of the past. The supposition that it is entails the view that one cannot have competent knowledge of anything unless one knows everything, and is a corollary to the philosophic doctrine of the "internality" of all relations. It will suffice here to note that, were the doctrine sound, not only would every historical account ever written be condemned as a necessarily mutilated and distorted version of what has happened, but a similar valuation would have to be placed on all science, and indeed on all analytical discourse. In short, the fact

that inquiry is selective because it originates in a specific and limited problem places the historian in no worse position than it does other scientists with respect to the possibility of achieving what is commonly characterized as objectively warranted knowledge.

2) Historical inquiry is selective not only in its starting point; it is also selective in proposing solutions to its problems. A variety of skeptical doubts about the possibility of an objective history has been expressed in consequence.

One such expression takes the form that, in view of the inexhaustibly numerous relations in which a given event stands to other events, no account can ever render the "full reality" of what has occurred. Accordingly, since every historical account covers only a few aspects of an occurrence and stops at some point in the past in tracing back its antecedents, every proposed explanation of that occurrence is said to bear the mark of arbitrariness and subjectivity. Part of this objection can be summarily dismissed with the reminder that it is never the task of any inquiry initiated by a specific problem to *reproduce* its subject matter, and that it would be a gratuitous performance were a historian in the pursuit of such a problem to formulate "all that has been said, done, and thought by human beings on the planet since humanity began its long career." Not only is the bare fact that inquiry is selective no valid ground for doubting the objectively warranted character of its conclusions; on the contrary, unless an inquiry were selective it would never come near to resolving the specific question by which it is generated.

However, the objection under discussion also rests on another misconception: it in effect assumes that since every causal condition for an event has its own causal conditions, the event is never properly explained unless the entire regressive series of the latter conditions are also explained. It has been maintained, for example, that

> A Baptist sermon in Atlanta, if we seek to explain it, takes us back through the Protestant Reformation to Galilee—and far beyond in the dim origins of civilization. We can, if we choose, stop at any point along the line of relations, but that is an arbitrary act of will and does violence to the quest for truth in the matter.[4]

But is there any violence to the truth? Is B not a cause of A simply because C is a cause of B? When some future position of a planet is predicted with the help of gravitational theory and information about the initial condition of the solar system at some given time, is there ground for skepticism simply because the assumed initial conditions are in turn the outcome of previous ones? These are rhetorical questions, for the answers to all of them are obviously in the negative. Moreover, precisely what is the problem in connection with the Baptist sermon in Atlanta? Is it why a given individual delivered it at a stated time and occasion, or why he chose a particular text and theme, or why that occasion happened to arise, or why Baptists flourish in Atlanta, or why they developed as a Protestant sect, or why the Protestant Reformation occurred, or why Christianity arose in antiquity? These are all quite different questions, and an adequate answer for one of them is not even relevant as a proposed solution for the others. The supposition that, when a problem is made definite a regressive chain of answers must be sought if any one answer is to be objectively warranted, is patently self-contradictory. On the other hand, the fact that one problem may suggest another, and so lead to a possibly endless series of new inquiries, simply illustrates the progressive character of the scientific enterprise; that fact is no support for the claim that unless the series is terminated, every proposed solution to a given problem is necessarily a mutilation of the truth.

Skepticism concerning the possibility of objectively warranted explanations in human history takes a more empirical turn when it bases its negations on the influence of personal and social bias upon such inquiry. The doubt embodied in the *aperçu* that history is written by the survivors is by no means a novelty; but in recent years it has been deepened and given a radical form by many sociologists of knowledge. According to some of them, all thought is conditioned and controlled by the "existential situation" in which it occurs; and, especially when thinking is directed to human affairs, the interpretation of observed facts, the selection of problems for inquiry and the methods employed for resolving them, and the standards of validity accepted are all functions of the thinker's unconscious value commitments and world outlook, his social position, and his political and class loyalties. Every cognitive claim concerning matters of vital human interest is therefore said to be valid only within the particular social setting in which it emerges; and the belief that it is possible to obtain explanations that are "true" for everyone, irrespective of his position in a given society, is declared to be part of the self-deception (or "ideology") of a culture.

There appear to be four distinct issues raised by this form of skepticism. In the first place, the choice of particular problems for study, especially inquiries into human affairs, is undoubtedly controlled by the character of a given culture, and

sometimes by the status of the student in that culture. An investigation of traffic problems is not likely to be made in an agricultural society, and a man's interest in labor history may very well be causally related to his social position. But, as has already been seen, this form of selective activity on the part of an inquirer does not necessarily jeopardize the objectivity of his findings.

In the second place, no inquiry takes place in an intellectual vacuum, and every investigator approaches his task with information and guiding ideas derived in large measure from his culture. But it does not follow from this circumstance alone that the conscious and unconscious value commitments associated with the social status of an investigator inevitably influence his acceptance of one conclusion rather than another. The preconceptions he brings to the analysis of a given problem may be neutral to all differences in social values, even when that problem is concerned with human affairs. And, in point of fact, there are many questions in the social as well as in the natural sciences upon which there is complete agreement among students, despite their different social positions and loyalties.

It is undoubtedly the case, in the third place, that the standards of validity operative in an inquiry are *causally* related to other cultural traits, and that social status, class and national bias, and general world perspectives frequently influence what conclusions a man accepts. For example, the degree of precision currently demanded in experimental work is certainly not independent of the current state of technology; and a comparison of Southern and Northern histories of the period of reconstruction following the American Civil War makes amply clear the force of sectional and race bias. This is an area of study that has not yet been systematically exploited, although sociologists of knowledge have already illuminated the genesis of many ideas and the manner in which social pressures enforce their acceptance. In any event, biased thinking is a perennial challenge to the critical historian of human affairs; and research into the causal determinants of bias is of undoubted value for recognizing its occurrence and for mitigating if not always eliminating its influence. The very fact that biased thinking may be detected and its sources investigated shows that the case for objective explanations in history is not necessarily hopeless. Indeed, the assertion that a historian exhibits bias assumes that there is a distinction between biased and unbiased thinking, and that the bias can be identified—for otherwise the assertion would at best be simply futile name-calling. In consequence, it is possible, even if frequently difficult, to correct the bias and to obtain conclusions in better agreement with the evidence. Accordingly, if doubt concerning the objectivity of a historical explanation is based on considerations relating to the causal influence of various social factors upon the evaluation of evidence, it is often salutary and well taken; but it does not entail a wholesale skepticism concerning the possibility of such explanations.

This brings me to the final issue. It is sometimes argued that the social perspective of a student of human affairs is not only causally influential upon his inquiry, but is *logically* involved both in his standards of validity as well as in the meaning of his statements. And it is also maintained that one must therefore reject the thesis that "the genesis of a proposition is under all circumstances irrelevant to its truth."[5] On the other hand, the radical skepticism concerning objective explanations of human affairs that results is qualified by the further claim that a "relational" type of objectivity can nevertheless be achieved. Thus, students who share the same social perspective and employ the same conceptual and categorical apparatus will allegedly arrive at similar conclusions on any problem when the standards characteristic of their common perspective are correctly applied. And students operating within different social perspectives can attain objectivity in a "roundabout fashion" by construing their inevitable differences in the light of the differences in the structures of their perspectives.

There are, however, grave factual and dialectical difficulties in these several claims. There is no factual evidence to show that the "content and form" of statements, or the standards of validity employed, are *logically* determined by the social perspective of an inquirer. The facts commonly cited establish no more than some kind of causal dependence between these items. For example, the once much-publicized view that the "mentality" or logical operations of "primitive" social groups are different from those typical of European civilization—a difference that was once attributed to institutional differences in the societies compared—is now generally recognized to be without foundation. Moreover, even the most extreme proponents of the sociology of knowledge admit that there are many assertions (those usually mentioned come from mathematics and the natural sciences) which are neutral to differences in social perspective and whose genesis is irrelevant to their validity. Why cannot assertions about human affairs exhibit the same neutrality? If, as no one

seems to doubt, the truth of the statement that two horses can in general pull a greater load than either horse alone is logically independent of the social status of the one who asserts it, what inherent social circumstance precludes such independence for the statement that two laborers can in general dig a ditch of given dimensions more quickly than either laborer working alone?

Second, what is the logical status of the claim that social perspectives enter essentially into the content and warrant of all assertions about human affairs? Is the claim itself meaningful and valid only for those occupying a certain social status? In that case, its validity is narrowly self-limited, no student with a different social perspective can properly understand or evaluate it, and it must be dismissed as irrelevant by most inquirers into social questions. Or is the claim peculiarly exempt from what it asserts, so that its meaning and truth are not logically dependent upon the social status of those who assert it? In that case, then, there is at least one conclusion about human affairs which may be "objectively valid" in the usual sense of this phrase; and if there is one such conclusion, there is no clear reason why there may not be others.

Finally, the relational type of objectivity which the claim admits as attainable is nothing other than objectivity in the customary sense, which the claim appears to deny as possible. A translation formula which renders the "common denominator" of seemingly diverse conclusions stemming from differing social perspectives, cannot in turn be "situationally determined" in the sense under dispute. Indeed, the search for such formulas is but a well-known phase of theoretical research in all areas of inquiry. It is a search for objective invariants in numerically and qualitatively distinct processes; and when the quest is successful, as it often is, it terminates in laws of greater or less generality, with whose help what is relevant to the occurrence of an event or to the continuance of a process can be distinguished from what is not.

In brief, therefore, although the historian is undoubtedly selective in the conduct of his inquiries, and although personal and social bias frequently color his judgment and control what conclusions he accepts, none of these facts precludes the possibility of warranted explanations for the events he studies.

III

The elimination of theoretical objections to the possibility of warranted explanations in history obviously does not ensure the realization of that possibility. As a matter of fact, there are serious obstacles, other than those already mentioned. which frequently do obstruct the quest for such explanations.

The search for explanations is directed to the ideal of ascertaining the necessary and sufficient conditions for the occurrence of phenomena. This ideal is rarely achieved, however, and even in the best-developed natural sciences it is often an open question whether the conditions mentioned in an explanation are indeed sufficient. Most historical inquiry is even further removed from this ideal, since the full circumstances are often quite complex and numerous and are usually not known. Historians therefore frequently cite only what they regard as the "main," "primary," "principal," "chief," or "most important" causal factors and cover their ignorance of the others by the convenient phrase "other things being equal." To mention but one example, the "main" cause of America's entrance into the first world war is declared by one careful student to be Germany's adoption of an unrestricted submarine warfare, though the factor cited is not assumed to be sufficient for producing the effect.

The "weighting" of causal factors in respect to their "degree of importance" is sometimes dismissed as essentially "arbitrary" and "meaningless"—partly on the ground that there is no warrant for selecting one occurrence as the cause of a given event rather than some prior cause of that occurrence (for example, since unrestricted submarine warfare was Germany's response to the British blockade, this latter ocurrence is allegedly as much the cause of America's entrance into the war as is the former), and partly on the ground that no verifiable sense can be attached to such characterizations as "chief" or "most important" in connection with causal factors. It must be admitted that the natural sciences do not appear to require the imputation of relative importance to the causal variables that occur in their explanations; and it is easy to dismiss the question of whether there is any objective basis for such gradations of variables, with a peremptory denial on the ground that, if a phenomenon occurs only when certain conditions are realized, all these conditions are equally essential, and no one of them can intelligibly be regarded as more basic than the others. And it must also be acknowledged that most historians do not appear to associate any definite meaning with their statements of relative importance, so that the statements often have only a rhetorical intent, from which no clear empirical content can be extracted. Nevertheless, we often

do make such claims as that broken homes constitute a more important cause of juvenile delinquency than does poverty, or that the lack of a trained labor force is a more fundamental cause of the backward state of an economy than the lack of natural resources. Many people might be willing to admit that the *truth* of such statements is debatable, but few would be willing to grant that they are totally without *meaning* so that anyone who asserts them is invariably uttering nonsense.

It is desirable, therefore, to make explicit what such statements may be intended to convey. In point of fact, ascriptions of relative importance to determinants of social phenomena appear to be associated with a variety of meanings, some of which I shall try to distinguish. If *A* and *B* are two adequately specified factors upon which the occurrence of a phenomenon *C* is supposed to depend in some fashion, the statements I wish to consider will be assumed to have the schematic form "*A* is a more important (or basic, or fundamental) determinant of *C* than is *B*."

1) *A* and *B* may both be necessary for the occurrence of *C*, though perhaps their joint presence is not sufficient for that occurrence. Then one sense in which *A* might be said to be a more important determinant of *C* than is *B* is simply this: variations in *B* occur infrequently and may be neglected for all practical purposes, whereas variations in *A*, with consequent variations in *C*, are quite frequent and perhaps uncontrollable. Thus, suppose that dislike of foreigners and need for economic markets are both necessary conditions for the adoption of an imperialist policy by some country; but suppose that xenophobia in that country varies little if at all during a given period, whereas the need for foreign markets increases. In this first sense of more important, need for foreign markets is a more important cause of imperialism than is dislike of foreigners.

2) But there is another though more difficult sense of more important. Assume again that *A* and *B* are both necessary for the occurrence of *C*. But suppose that there is some way of specifying the magnitude of variations in *A*, *B*, and *C*, respectively, and that, although changes in one may not be comparable with changes in another, the changes within each item are comparable. Suppose, further, that a greater change in *C* is associated with a given proportional change in *A* than with an equal proportional change in *B*. In that event, *A* might be given a more important rank as a determinant of *C* than is assigned to *B*. For example, assume that a supply of coal and a trained labor force are both necessary for industrial productivity; but suppose that, say, a 10 per cent variation in the labor force produces a greater alteration in the quantity of goods produced (as measured by some convenient index) than does a 10 per cent variation in the coal supply. Accordingly, the availability of a trained labor force could be said to be a more important determinant of productivity than the availability of coal.

3) Suppose now that the joint presence of *A* and *B* is not necessary for the occurrence of *C*, so that *C* can occur under conditions *A* and *Y*, or under conditions *B* and *Z*, where *Y* and *Z* are otherwise unspecified determinants. In this case, also, there is a sense of more important analogous to the first sense mentioned above. More explicitly, the frequency with which the first condition *B* and *Z* are realized may be small when compared with the frequency of the realization of *A* and *Y*; and this possibility may then be expressed by saying that *A* is a more important determinant of *C* than is *B*. Thus, assume that automobile accidents occur either because of negligence or because of mechanical failure; and suppose that the frequency with which there is such failure that leads to accidents is very much less than the frequency with which carelessness terminates in accidents. In that case, negligence may be said to be a more important cause of accidents than is mechanical failure.

4) Assume, again, that the joint presence of *A* and *B* is not necessary for the occurrence of *C*; and suppose that the relative frequency with which *C* occurs when the condition *A* is realized but *B* is not is greater than the relative frequency of *C*'s occurrence if *B* is realized but *A* is not. It is such a state of affairs which is sometimes intended by the assertion that *A* is a more important determinant of *C* than is *B*. For example, a statement such as that broken homes are a more fundamental cause of juvenile delinquency than is poverty is frequently best interpreted to mean that the relative frequency of delinquency among juveniles coming from broken homes is much greater than among children coming from homes marked by poverty.

5) One final sense of more important must be mentioned. Suppose that a theory *T* is formulated with the help of *A* as a fundamental theoretical term; and suppose that *T* can account for the phenomenon *C* when *T* is supplemented by appropriate data which involve reference to *B*. In consequence, though reference to *B* is essential for explaining *C* with the help of *T*, reference to *B* is not always necessary when *T* serves to explain phenomena other than *C*. Accordingly, since the range of phenomena which fall within the province of *T* (and therefore within the range of application

of A) is more inclusive than the phenomena for which B is relevant, A may be said to be a more basic determinant of C than is B. Something like this sense of more basic appears to be intended by those who claim that the social relations that govern the production and distribution of wealth constitute a more basic determinant of the legal institutions of a society than do the religious and moral ideals professed in that society.

Other senses of more important or more basic can undoubtedly be distinguished, but the five here mentioned appear to be those most frequently used in discussions of human affairs. It is essential to note that, although a definite meaning may thus be associated with ascriptions of greater importance to assumed determinants of social processes, it does not follow that the available evidence does in fact warrant any given assertion of such a claim. Accordingly, even when a historian does intend to convey a verifiable content by such assertions, it is doubtful whether in most cases they are actually supported by competent evidence. There is next to no statistical material bearing on the relative frequency of occurrence of the phenomena of special concern to students of human affairs. Historians are therefore compelled, willy-nilly, to fall back upon guesses and vague impressions in assigning weights to causal factors. There are often wide divergences in judgment as to what are the main causes of a given event, and one man's opinions may be no better grounded than another's. Whether this defect in current causal imputations in historical research can eventually be remedied is an open question, since the probable cost of remedial measures in terms of labor and money seems staggering. Meanwhile, however, a judicious skepticism concerning the warrant for most if not all judgments of relative importance of causal factors (among those assumed to be relevant to an event) appears to be in order.

Doubtless the basic trouble in this area of inquiry is that we do not possess at present a generally accepted, explicitly formulated, and fully comprehensive schema for weighing the evidence for any arbitrarily given hypothesis so that the logical worth of alternate conclusions relative to the evidence available for each can be compared. Judgments must be formed even on matters of supreme practical importance on the basis of only vaguely understood considerations; and, in the absence of a standard logical canon for estimating the degree in which the evidence supports a conclusion, when judgments are in conflict each often appears to be the outcome of an essentially arbitrary procedure. This circumstance affects the standing of the historian's conclusions in the same manner as the findings of other students. Fortunately, though the range of possible disagreement concerning the force of evidence for a given statement is theoretically limitless, there is substantial agreement among men experienced in relevant matters on the relative probabilities to be assigned to many hypotheses. Such agreement indicates that, despite the absence of an explicitly formulated logic, many unformulated habits of thought embody factually warrantable principles of inference. Accordingly, although there are often legitimate grounds for doubt concerning the validity of specific causal imputations in history, there appears to be no compelling reason for converting such doubt into wholesale skepticism.

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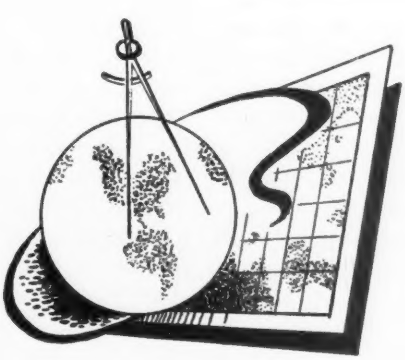

One Hundred Years of British Science

MAURICE GOLDSMITH

For several years Maurice Goldsmith, Unesco science editor at Unesco's Paris office, has furnished AAAS members with accounts of the annual meetings of the British Association for the Advancement of Science. It has been customary to publish his lucid reviews of these convocations in SCIENCE, *but the theme of the Edinburgh Meeting of August 1951—"The British Contribution to Science and Technology in the Past Hundred Years"—makes his report especially appropriate for* THE SCIENTIFIC MONTHLY.

THE 113th annual meeting of the British Association for the Advancement of Science was held at Edinburgh in August 1951. It provided the occasion for a review of British achievement over the past one hundred years, and for an official statement on the social function of science. The meeting coincided with the national celebrations that took place throughout Britain during the Festival Year.

In his presidential address on "The British Contribution to Science and Technology in the Past Hundred Years," H.R.H. the Duke of Edinburgh recalled how almost a century ago his great-great-grandfather, the Prince Consort, had referred, in his address to the British Association, to "the high position which Science occupies, the vast number of distinguished men who labour in her sacred cause, and whose achievements, while spreading innumerable benefits, justly attract the attention of mankind." The Duke, who felt that "just as an outsider, a layman so to speak," he could be useful to science, regarded his own address as largely the story of the fulfillment of his distinguished ancestor's hopes. The invitation to him to speak seemed "to demonstrate that Science is not a magic circle and that you wish us to enter your confidence."

Between 1851 and 1870, practice in many industries was ahead of science. The great stimulus of the 1851 exhibition resulted in the widespread improvement and application of the inventions—mainly the work of British genius—of the industrial revolution. Britain is now still struggling with the social results of this vast expansion.

From 1870 to 1890 the high-water mark of British industrial expansion was reached, and the competition of the United States and Europe was beginning to be felt. But the lack of serious competition hitherto had bred a feeling of overconfidence and satisfaction in the methods and processes employed, which resulted in a conservative attitude toward technical change. Concurrently, a subtle change occurred in the type of British exports, from products of machinery to machinery itself, which intensified foreign competition between 1890 and 1914.

The critical years of the first world war brought a realization of the part science must play in the industrial and military strength of the nation and led to the founding of the Department of Scientific and Industrial Research. After the war there was a rapid expansion of industrial research, and many new commercial research laboratories grew up. British dependence on foreign production of vital articles such as dyestuffs, scientific instruments, and optical glass was remedied by the help of the key industry import duties, which gave support to the establishment of these industries in Britain.

The period from 1914 to 1939 saw rapid development in the internal-combustion engine, oil technology, and in the electrical, chemical, and aircraft industries, as well as in electronics, synthetic fibers, plastics, aerodynamics, and light alloys. In the second world war science was from the outset completely coordinated with the war effort, which showed up out-of-date industrial capacity and initiated comprehensive reconstruction on modern lines. Since the war the almost complete absence of income from foreign investments has forced Britain to rely once more on her capacity to manufacture; although physical sources have dwindled, the intellectual capacity of British engineers and scientists is as great as ever.

During the century since 1851, the contribution of the British Commonwealth to natural science has been of outstanding importance.

In astronomy, the work of Eddington, Jeans, and Milne on mass luminosity and stellar evolution, Huggins' application of spectrum analysis to

astronomy, and Lockyer's discovery of helium in the sun were of the greatest significance. The work of Abercromby and Shaw on the behavior of the earth's atmosphere in the troposphere started the scientific study of weather and weather prediction, and Appleton's research into the ionosphere extended this to the upper air.

In chemistry, Crookes broke fresh ground by his work on spectra and his discovery of thallium and of cathode rays. Rayleigh started the hunt for inert gases by discovering another ingredient, which he called argon, in the air, and Perkin and Robinson added to the knowledge of the structure of carbon compounds and the power to copy natural products synthetically. X-ray analysis was developed by the two Braggs, father and son.

In physics, Thomson's discovery of the nature of the electron was the first attack upon the integrity of the atom. Rutherford was the first man to succeed in the transmutation of an element, and evolved the nuclear theory of matter.

In technology, scientific metallurgy might be said to have started when Sorby first applied a microscope to the surface of metals, and the great majority of the mechanical developments of the period were due to new alloys that could withstand higher stresses. Parsons' invention of the steam turbine revolutionized large-scale power production on land and sea, and Froude's work on hull forms and propellers allowed the full benefit of the new prime movers to be reaped at sea. Lanchester's vortex theory was one of the steppingstones to powered flight, and Alcock and Brown made the first transatlantic flight in 1919. Of outstanding importance was the genius of Mitchell in aircraft design, and, more recently, Whittle's pioneer work gave Britain the lead in jet engine production for both civil and military use.

After Faraday, who belongs to another period, the second founder of electricity and electronics was Clerk-Maxwell, with his classic treatise on electromagnetism. Other contributions of great importance were made by Wilde, who developed the dynamo; Swan, the incandescent lamp; Wheatstone and Kelvin, who pioneered the use of electricity for communication; Heaviside and Appleton, who made discoveries in the propagation of radio waves; Watson-Watt, who developed radar; and Randall, whose use of the magnetron for high-frequency radar was one of the major contributions to the Allies' equipment for war. Baird's name will always be linked with the first successful television pictures.

The story of plastics and synthetic materials started with Parke's discovery of celluloid, and Cross and Bevan's manufacture of viscose, which gave birth to the rayon industry and the many later types of synthetic fiber.

Darwin's *The Origin of Species* and *The Descent of Man* dominated biological science, to which Francis Galton and William Bateson in the field of heredity, and Sherrington, Dale, and Adrian in that of the nervous system, made important contributions. Gowland Hopkins founded the science of biochemistry in Britain, and his discovery of vitamins started the modern science of nutrition.

Fleming discovered the antibacterial properties of penicillin, and Florey and Chain, at Oxford, found that penicillin could be extracted in a highly purified form and used to treat human disease. Simpson's use of chloroform, Lister's antiseptic, and Macewen's aseptic techniques formed the basis of modern surgery, and in tropical medicine Britain led the world through the work of Manson, Ross on malaria, Bruce on sleeping sickness, and Finlay, Adrian Stokes, and Hindle on yellow fever.

In the field of nutrition, the Food Investigation Laboratories of the Department of Scientific and Industrial Research have since 1918 established the biological knowledge on which the storage and transport of meat, fish, and fruit are now largely based. Rothamsted, the oldest agricultural research station in the world, was founded by Lawes, who by discovering superphosphate started the fertilizer industry. Biffen's work in plant breeding at Cambridge became one of the great contributions toward feeding the world's growing population, and the investigations of Cossar, Ewart, and Crew advanced the study of animal breeding. Britain also played a leading part in the mechanization of agriculture.

Although the foundations of psychology were laid elsewhere, important contributions were made by men like Ferrier, Bain, and Ward; and Sully's work on child psychology was the first of its kind. Galton's studies of the mental differences among individuals have been widely respected in all psychological laboratories.

Possibly the most vital factor affecting the industrial application of scientific research has been the lack of a coordinated system of scientific and technological education in Britain. Excellent though they are, the existing institutions, which have grown up to meet particular circumstances, have not produced enough trained technologists to meet the urgent needs of scientific development in industry and to provide leaders for the future.

We have evolved a civilization based on the material benefits which science and technology could provide. The present shortages of food and raw materials were a timely

reminder of the slender material foundation on which our civilization rested, and of our dependence upon science and technology.

The pursuit of truth in itself cannot produce anything evil. It is in the later stage when the facts dug up enter the process of application that the choice between the beneficent and destructive development has to be made. It is quite certain that it is an exception if any particular discovery cannot be used equally well for good and evil purposes. Happily the beneficent exploitation of scientific knowledge has kept pace with its destructive application.

To my mind [the Duke continued], it is vital that the two sides of scientific development are fully and clearly understood, not only by the research scientist, inventor, designer and the whole scientific team, but also by all laymen. The instrument of scientific knowledge in our hands is growing more powerful every day; indeed, it has reached a point when we can either set the world free from drudgery, fear, hunger and pestilence or obliterate life itself.

Progress in almost every form of human activity depends upon the continued efforts of scientists. The nation's wealth and prosperity are governed by the rapid application of science to its industries and commerce. The nation's workers depend upon science for the maintenance and improvement in their standard of health, housing and food. Finally, superiority or even our ability to survive in war is a direct measure of the excellence and capacity of the scientific team.

This team of research workers and engineers has a dual responsibility, one for its work and the other as informed citizens, and it can only fulfill its proper functions if its members have a sound general education as well as a thorough training in science. It is no less important that the people who control the scientific machine, both laymen and scientists, should have a proper understanding and appreciation of what science has grown into and its place among the great forces of the world.

It is clearly our duty as citizens to see that science is used for the benefit of mankind. For, of what use is science if man does not survive?

Each of the presidential addresses given to the thirteen different sections was in the main a summing up of the situation at this mid-century.

Cyril Hinshelwood, president of the Chemistry Section, found the chemist, after a long and eventful approach, standing face to face with the problem of life itself. The evolution of the central theory of chemistry—the atomic and molecular theory—served, he said, to reflect some of the profound changes in thought and intellectual attitude that had in the past few generations come over science as a whole. The atomic theory in its inception explained the quantitative laws of chemical composition, but it gave no clue, in its early form, to the nature of affinity, though naturally and properly people began to ask the naïve question: Why do atoms combine?

On the basis of the modern mathematical laws, calculations of behavior can be made, but the laws themselves are, in their present form, strangely unsatisfying to the minds of many. They seem almost to deny the possibility of the deeper kind of understanding. They present to the inquirer that austere negation which the philosophy of logical positivism seeks to impose in a wider sphere.

Observable phenomena may, according to that grim code, be correlated, but the kind of satisfaction which men of all centuries have looked for and called understanding must be renounced as a frivolous desire for something that does not exist. Mind and matter in philosophy at large, individual ultimate particles in physics and chemistry, so far from being immutable essences, become fictions providing illusory answers to improper questions.

So far from accepting this bleak doctrine [Sir Cyril continued], I think we may well stand on the threshold of even more surprising changes. If our present notions of particles and forces have involved us in difficulties and contradictions, then the plain inference is that we must go on thinking. [From the nature of the case he could not foretell what the resolution of the present dilemma would be, but there was one reflection on the subject which it might be well to make.]

A surprising amount of the structure of chemistry is now seen to depend upon the famous Pauli principle, which may be said crudely to recognize different kinds of electron, but not different electrons. Behind this lies some deep mystery relating the possibility of detection with the very existence of the entities postulated in physical theories.

One sees there some connection with the mind-matter mystery itself, but all attempts to say something helpful about the relation of those two interpenetrating but apparently immiscible worlds have so far led to contradictions, frustrations, and absurdities. Clearly there is a mind-matter problem, and thoughtful people will go on wrestling with the question.

The gifts of science, we were told, are in danger of being utilized not for the benefit but to the detriment of mankind, and voices are even heard which call for the restraint and repression of discovery. Men of science are sometimes represented as blind to higher values and as loosing upon an innocent world agents of untold destruction. "This charge seems to me as grave as it is misguided," Sir Cyril said. What is the power of destructive weapons compared with that of lying propaganda, which can destroy men's souls? And was that product of the devil begotten by a man of science? he asked. To wish to inhibit or restrict scientific discovery shows an utter lack of faith in human destiny. "If there is any controversy about this, then it is not men of science who are blind to higher values, and in any case it would be preferable merely to be blind to higher values than to use an arrogant personal conception of them to stifle the pursuit of truth by others."

The man of science in general—the chemist in particular—has to protect himself in the future against two menaces: he is in peril of being swept away into a Sargasso of administrative seaweed; he is constrained to forsake the laboratory for the conference hall and board room, and as a result a temporary decline in fertility in the near future is almost inevitable. And on the scientific side there has grown up a thicket of specialized detail, with a loss of wider prespectives; that, too, is temporary and will pass as broader syntheses are constructed and as new and more powerful theories succeed further in the tasks of coordination and prediction.

W. Brown, in his presidential address to the Botany Section, confined himself in the main to tracing the advancement of the mycological branch of botany over a century, which happened to represent very accurately the lifetime of mycology (and bacteriology) as a science. We are now witnessing the growth of a fungal genetics which has already achieved important theoretical and practical results. There is even on the way a cytogenetics of fungi with sex linkage, lethal factors and crossing over, in spite of the difficulties arising from the smallness of their nuclei. By a process of crossing, new groupings of characters can be produced, more or less at will.

Hence the possibility, and in fact the actuality, of improved races of fungi for economic purposes, as has been recently shown with strains of yeast for brewing and of *Penicillium* for penicillin production. There are also dangerous possibilities, as in the appearance of more virulent strains of pathogenic organisms, either by mutation or by natural hybridization, or, according to some workers, by a chemical process of adaptation. Examples that illustrate that danger are numerous and continue to increase; two such of recent origin are the strains of the potato blight fungus that are able to attack hitherto immune varieties of the host, and the strains of *Streptococcus* that are resistant to penicillin.

Although it might be said that most plant physiological problems, with the notable exception of photosynthesis, can be studied in the fungi, they particularly lend themselves to the study of respiration. Weight for weight, fungi and bacteria are among the most actively respiring organisms known, in conformity with their function in nature as agents for breaking down organic material; they also offer a greater range of respiratory processes—from aerobic to strictly anaerobic, in addition to special types, as in the conversion of ammonium salts to nitrate, sulfide to sulfate, ferrous salt to ferric.

Looking ahead, one might guess that the future of mycology and of biology generally will lie in physiological advancement, and that in time might have an impact upon morphology. The physiology of the future will probably become more and more biochemical; therefore cooperation is essential between the biologist and the biochemist, and it will be made easier and more fruitful if biological training is orientated more in a physiochemical direction, especially in view of the rapid advances taking place these days in fundamental aspects of physics and chemistry.

Addressing the zoologists, C. F. A. Pantin, president of the section, showed that in the last generation work on the living animal and on its chemistry and physiology has at length brought them back to the central problem on organic design. The principles that emerged, he said, were not those emphasized by the phylogenetic morphologists of the end of the last century. They do not contradict evolutionary principles: they are additional to them. They apply to all grades of structure, from the molecule up to the complex computing network of the central nervous system. Zoologists recognize that each functional problem before the organism admits of certain possible forms of solution. To meet one or the other of those solutions, structural systems can be built by utilizing the unique emergent properties of the molecules and higher orders of structural unit that are available.

Natural selection, said Dr. Pantin, will determine which of the various possibilities actually survives. It is therefore not molding an infinitely plastic organism; it is rather directing it from one possible state to another, somewhat after the fashion of the moves in a game of chess. Although recognizing an abstract plan, it is not a plan peculiar to living things, he said; there is nothing vitalist about it. The plan emerges from the unique properties of matter and energy, and even its more complex consequences govern the constitution of inanimate objects like calculating machines as well as living systems.

Pindar wrote at the dawn of the fifth century B. C.: "The wise know the wind that shall blow on the day after tomorrow, and are not wrecked through eagerness for gain." In so writing, said David Brunt, in his sectional presidential address on "A Hundred Years of Meteorology (1851–1951)," Pindar set up a standard of weather forecasting which even today is difficult of attainment, except on rare occasions. In 1851 the general distributions of pressure, temperature, and wind over the globe were known at least in outline; the

distribution of weather in depressions was known from the study of individual depressions; the rate of fall-off of temperature with height in the lower layers was appreciated; the mode of formation of clouds was understood, though there would appear to have been differences of opinion concerning the formation of air.

Even today weather forecasting is an extremely complicated science, and any improvement in forecasting is to be looked for in the treatment of the atmosphere as a three-dimensional medium, and finding methods of representing the distribution of temperature and the other variables, in three dimensions, on a system of two-dimensional charts. In the postwar era the much-enlarged meteorological services in all the larger countries of the world have led to greater effort to solve some of the fundamental problems of meteorology. Among those that have received increased attention are: (*a*) The nature of the depression of middle latitudes, "but we are not yet in a position to predict where the next depression will form, or even to explain the precise conditions which led to the formation of yesterday's depression;" (*b*) the effects of atmospheric radiation and absorption on the heat balance of the atmosphere; and (*c*) the formation of cloud, rain, ice crystals, and snow.

In his presidential address to the geographers, O. J. R. Howarth said that Adam and Eve must have loved their Garden of Eden. "Such earth-love must always have been a primitive, deep-seated instinct of man: we are, if you care to coin a word, geophilists." Of that love were born farmers and travelers and geographers, including such enthusiasts as the advertiser in *The Times* not long ago, "the six-foot geographer (female)," who asked for an "adventurous post." But in some that love of the land is absent or latent because of the setting of their lives in places where the beauties of the land are not seen. To teach the beauties of the land for the sake of their preservation must surely be a high function of geography.

It is not given to every age to witness the birth of a new profession, declared C. A. Mace in his presidential address to the psychologists; but in the century between the two exhibitions there had emerged in this country not only the science but also the profession of psychology.

He believed that the profession of psychology now stands at the parting of the ways. One of these ways has been taken by the medical profession, which on the whole has decided that its public should not be informed. The other way is that taken by the ministry of our immortal souls, which has encouraged every layman to understand the Gospel, each to the limit of his powers of comprehension. Between those two ways the professional psychologist has now to make his choice. "For my part," said the president, "I have no doubt what this choice should be. . . . A policy of collaboration with the laity is, I believe, right, not only for the profession of psychology but for the science, too. British psychology has, in this respect, a great tradition to preserve."

Referring to the need for closer association between professionals and laity in the arts, the sciences, and the technologies, Professor Mace spoke of the present need for advance in higher technological education. You have only to scratch the surface of the problem, he said, to find issues big enough to interest and to embroil not only technologists and industrial psychologists but other scientists as well, and not only scientists but also professors in the humanities, and not only all those professionals, but also the laity of the arts, the sciences, and the technologies and those who enjoy experience relevant to those issues.

It is a common belief in this country that to carry out pure scientific research or to study classics or history is a higher calling than to apply knowledge to productive industry. To understand and to remedy that fundamental weakness of our civilization would be a worthy object of prolonged research. It would be a bad mistake to dismiss the fact as yet another instance of human snobbery. He believed, Mace said, that both the diagnosis and the remedy lie in fundamental research into basic conditions of "status" and "prestige."

Emphasizing the need for a liberal education, which he defined as "one that secures the greatest areas of contact between the greatest number of different ways of life," Professor Mace said that two corollaries might be developed. First, psychologists have a special responsibility for the maintenance of the British tradition in the popularization of science. It is in that tradition not only to maintain the highest standard of scientific journalism, but also for those who themselves make discoveries to present them to the laity.

The second corollary is that psychologists have a special responsibility for the preservation of a common medium of communication. The recent demands for the use of "plain words" and of "direct English" are not solely for utility or aesthetics; they are of sociological, anthropological, and psychological significance.

In his sectional presidential address on "Experimental Agriculture," E. M. Crowther said that the present time is again a period of rapid techno-

logical and economic change, with an expanding demand for agricultural products and increasing costs of production. In many parts of the world agricultural pioneers, who are now often the officers of agricultural departments, research institutes, or corporations, are trying to forecast how soils and crops will behave under new agricultural systems, for which there is little or no local traditional experience. It is axiomatic that new developments must safeguard the productivity of the land or, in other words, that soil fertility must be maintained or, still better, enhanced. Difficulties often arise in applying that maxim to specific cases.

On the subject of research, Dr. Crowther declared it is essential that field experimental work should not be wasted on matters of purely local and temporary concern, but should be planned to make the fullest use of, and to provide the best raw materials for, investigations in other branches of agricultural science. The problem of balancing the various lines of approach might be simplified, in research and in teaching, if it were realized that agricultural science does not seek to establish principles from which agricultural practices might be deduced, but has a more modest aim.

Agricultural science might be regarded as a search for patterns of performance to be fitted to suitable landscapes; its starting point is current practice in the region concerned or in a similar one. The features of many environments and the effects of most agricultural operations can be classified sufficiently well for forecasting the effects of moderate changes in practice, but it is unwise to make drastic innovations without pilot schemes and detailed experiments.

In his presidential address on "The Physiology of the Capillaries," H. P. Gilding said that in spite of the attention that has been paid to the circulation as a whole it must strike any serious student of physiology as odd that so little is understood about the *raison d'être* for the circulation—namely, the exchanges that must continuously go on if life is to be maintained.

People have thought of the capillaries—with their exquisitely thin walls (less than one micron)—as conveniently placed channels through which filtration can occur, and tend to forget that they are made up of living cells with a membrane, or rather two membranes, through which substances must pass. Should we not get further in our understanding if we regarded those cells and their permeability as being subject to the same physicochemical laws as other cells in the body—remembering that internally they are exposed to a small hydrostatic pressure and that their inner and outer membranes are much closer together than most other cells, and further, in common with other living cells, can alter their permeability to meet the requirements of the moment?

May it not be also that the endothelial cell, normally placed to have the first pick at any available oxygen, is unable to carry out and control its function of tissue exchange in circumstances when production of energy is depressed?

The problem of ensuring that education for all, up to the age of eighteen, when it eventually arrives, will be a means of genuine gain and not boredom and frustration was discussed by Hector Hetherington, principal and vice chancellor of the University of Glasgow, in his presidential address to the Education Section. He did not doubt, he said, that the experience of the past hundred years had disclosed some of the conditions that a developing civilization must seek to fulfill; and that we had not mistaken the way to a more humane society in which men could be more themselves and more content in their service. The proof of it is that in considerable measure the deep divisions of our society have been overcome. It is true, in a sense in which it was not true in 1851, that we are one people.

The basic principle in education today is universal full-time and effective schooling from five to fifteen—and soon to sixteen years—with, though not yet realized, compulsory part-time schooling until eighteen, and voluntary attendance before and after the ages of obligation. Within that scheme is enshrined the principle of a genuine secondary education for all.

> We have come to the threshold of opportunity [he said]. We have still to show that we can use it. A longer school life: secondary education for all. These may be the means of a genuine gain—will be, if we employ them with understanding and imagination.
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> Or they may be the means of boredom and frustration: in which event, our second state will be worse than our first. It will take boldness and imagination and methods other than those with which we are familiar.

People have to find or create within a modern society an experience which in some sort gives meaning and purpose to their lives. "That is the deepest need of all. A community is not secure until its members feel that as their society calls upon their service so, as far as may be, in a measure not widely disproportioned to what they give, it opens to them access to those interests and activities which are the constituents of a good life." He did not doubt, he continued, that the classics retained their ancient power, but happily they have no monopoly.

Sir Hector laid emphasis on the need, in the

schools, of shaping the scale of values among the scholars, and of helping the young to discover where the rewarding experiences lie.

I have [he said] known men of my own land, mostly, alas! elderly, and most of them country-bred, of no great schooling and no wide accomplishment, who were yet, and enviably, educated men. They knew the ways of Nature, and they knew their Bibles. They had no rubbish in their minds: they thought and spoke with wisdom and dignity.

Exposure to the best. That is the way to the possession of standards and to a scale of values. Let us where we can use the way of the great books, but we have to seek it, too, in other ways, especially through the discipline of practical learning For that is the very heart of the challenge, with which we, as educators, are faced.

We have to engender not only the skill and the will to do, but the conviction that the doing is itself rewarding. Whatever the instrument—solving equations, judging the quality of timbers, using a lathe, playing a fiddle—we miss the summit of it all if there be no moment when the mind of the learner takes fire, when he has sight of a kind of perfection attainable in this activity, so that here at least he has hold of an ideal standard, and wants with all his heart to do this as well as it can be done.

By that way comes integrity of judgment which is an element in integrity of character. I know no other. No matter that a man cannot himself be a master. Few of us can, in anything. But he knows the difference between the first-rate and the second-rate; he knows what excellence is, that it is worth having, that it is costly and not to be had for the asking; and he is by that the less at the mercy of the abounding cheap and the shoddy.

"Only a small improvement is required in the method of using heat generated by nuclear fission to enable atomic energy to compete with present-day practice," declared Claude Gibb, president of the Engineering Section.

"To justify the high initial cost of an atomic energy plant," he went on, "the production of electrical energy by fission must be carried out on a large scale, and so electrification of railways and other large-scale usage of electricity would result automatically."

Sir Claude made some other predictions of the possible outcome of developments now in their infancy. Transport will always be a major item in civilized communities, and there seems little doubt that the tendency in the future will be toward large-scale electrification in those countries where population and traffic density conditions will allow this to be carried out economically. This tendency may well be hastened by the application of nuclear energy, on a large scale, to the production of electrical energy.

The production of electrical energy from fissile material is, even in the light of our limited experience today, a practical possibility.

It is unlikely that in the future our available supplies of coal will increase; on the contrary, they are likely to become less year by year. Economics will therefore force us to use coal more economically and I foresee high pressure gas mains supplying heat to home or industry, the complete abolition of the open coal fire, the increased use of slow combustion heating in homes using coke or other residual fuels, gas turbines living up to their names and using gas, and the use of coke, gas or tar, and of course, high ash coal residues for generating electricity in central power stations.

A great deal has already been done in the blending of coking and non-coking coals for gas production and I expect work of this nature will be intensified so that the total output of coke will be increased and valuable by-products obtained.

Work is now being undertaken in this country in burning methane, which occurs in small proportions in the large volumes of air used in mine ventilation, and it may be that in the future, by using a high degree of pre-heat, prior to combustion, the miner's greatest enemy—firedamp—will be used for the benefit of man.

Without any doubt, in my opinion, the jet or gas turbine cum jet will be used to the exclusion of all other means of propulsion for all types of aircraft. This type of prime mover meets the requirements so admirably that it is difficult to see how any other form of power unit can compete. On land, the gas turbine is more sorely pressed by its competitors and I doubt if any prime mover other than the steam turbine, will be used for very large powers. I do think, however, that the gas turbine will, to a great extent, supplant the diesel engine. There is no difficulty in making gas turbine units of 100 H.P. which could easily be fitted to motor cars within the space now occupied by petrol or diesel engines. The major remaining problem requiring solution to enable the gas turbine to take its place in our everyday life is that of a compact, low cost and highly efficient regenerative heat exchanger. The consideration given to this problem during our present meetings is an indication of its importance and likely solution. Once a purely rotating prime mover has been found suitable for a given application, it always becomes possible to supplant an alternative reciprocating type, and there is little doubt but that within the next fifty years, the gas turbine driven motor car will be used to the exclusion of all others.

Man's standards of living and comfort have increased only as he has made mechanical power his aide and servant in increasing degree. Electricity provides the most convenient way of supplying that mechanical power and if the nations of the world are listed in the order of electricity consumption per head of population it will be found to coincide with the nations listed in the order of living standards. Incidentally, nations listed in the order of protein percentage in their daily diet give again the same sequence. Inevitably as nation after nation strives for improved standards of living and comfort, there must be a tremendous expansion in the generation and use of electricity. A consumption per head in Great Britain of five times our present electricity usage still would not produce demand saturation.

Large-scale use of electricity for pump-produced rain and for soil heating as a means of combating in some degree the vagaries of weather and thus help solve our evergrowing food shortages is certain to come.

Supersonic frequency electronically produced vibrations will become an everyday thing in our industrial, domestic and medical life. The thermionic valve will become an increasingly integral part of our life between 1951 and 2051.

Of great interest was a paper read by Max Born, Tait professor of natural philosophy at Edinburgh University, on "Physics in the Last Fifty Years." He began by saying that he learned classical physics in his youth at Breslau and experienced the revolutionary character of Einstein's relativity theory of 1905. Eight years later came Einstein's second paper on general relativity. This paper was based on an elementary but, significantly, so far unexplained fact—that all bodies fall with the same acceleration. "To this day," said Professor Born, "it is this empirical foundation which I regard as the cornerstone of the colossal mathematical structure erected on it."

The field equations are, he thought, more convincing than the confirmation of the astronomical prediction of the theory. They lead to a renewal of cosmology and cosmogony on an unprecedented scale. Bigger telescopes have been built. Today the age of the world has been determined, and there is good reason to assume that the laws of physics are the same throughout the universe. However, the fundamental problem of connecting gravitation with other physical forces, to explain the strange value of the gravitational constant, is still unsolved in spite of Eddington's obstinate, ingenious attempts and of the claims of a group of his pupils in Cambridge to have solved all the riddles of the universe.

Professor Born said that the most promising idea seemed to be that of Dirac, developed by Jordon, that the gravitational constant is not a constant at all but a scalar field quantity which undergoes a secular change—though exactly what that is perhaps only physicists know. However, the concepts of classical physics still exist "even in America, where it might be thought that the only decent pursuit deserving the name of physics was nuclear physics."

> On nuclear fission [he said] the political and economic implications of these developments are too formidable to be discussed here. But I cannot refrain from saying that I am personally glad not to have been involved in the pursuit of research which has already been used for the most terrible mass destruction in history and threatens humanity with even worse disaster.
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> I think the application of nuclear physics to peaceful ends is a poor compensation for these perils . . . however, the human mind is adaptable to almost any situation. So let us forget for a while the real issues and enjoy the most useful results obtained from the pile.

Professor Born concluded by saying that, in his opinion, the most important contribution to the natural philosophy of the last decade was the discovery of the meson (theoretically predicted by Yukawa, of Japan, in 1935). "It becomes more and more clear," he said, "that even the latest mathematical refinements do not enlighten the theory of quantised fields and that a far more general theory has to be found in which a new constant—an absolute length or time or mass—appears and which ought to account for the masses found in nature."

A. S. Parkes, of the National Institute for Medical Research, in a paper on "Preservation of Spermatozoa at Low Temperature," gave details on some new work in methods of preserving spermatozoa in vitro for indefinite periods. The introduction of artificial insemination has greatly increased the potential reproduction capacity of the male, but the inability to preserve spermatozoa in functional condition for more than a few days still results in serious waste of such capacity.

Of the several major factors bearing on the survival of spermatozoa in vitro, temperature has long been recognized as one of the most important. Spermatozoa kept in vitro at temperatures similar to that of the body rapidly exhaust themselves, and their period of survival is limited. In many cases a temperature slightly above zero is optimal for preservation by present methods. Bull spermatozoa, for instance, cooled slowly, retain their fertilizing power for several days at about 4° C. Temperatures slightly below zero may be tolerated under certain circumstances, but solidification of the semen is very injurious to the spermatozoa, an effect usually ascribed, perhaps rightly, to the formation of ice crystals.

Human spermatozoa are unique in their resistance to freezing. With the spermatozoa of other species, special techniques are necessary if even a small proportion are to be revived after exposure to low temperatures.

Dr. Parkes' colleagues, Dr. Smith and Mr. Polge, made the discovery that glycerol has remarkable properties in protecting fowl spermatozoa against the effects of low temperatures. If fowl semen is diluted with equal parts of Ringer's solution, frozen at – 79° C for 20 minutes, and then rapidly thawed, no significant revival of spermatozoa is observed. On the other hand, if the dilution is carried out with Ringer's solution containing 40 per cent glycerol the spermatozoa resume full motility on thawing. So far as retention of motility is concerned, the specimen is indistinguishable from its unfrozen control. Specimens of spermatozoa have been found to resume motility completely after long periods (up to 5 months) at – 79° C.

> We then turned our attention to applying this discovery to the preservation of mammalian spermatozoa, but significant revival was not obtained after the compara-

tively rapid freezing achieved by plunging a test-tube containing 1 cc. of spermatozoal suspension into alcohol cooled to −79° C. It was then found, however, that with a final concentration of 20% glycerol slow cooling at the rate of about 1° C. per minute from 0° C. to −79° C. was compatible with the revival of a large proportion of spermatozoa of the bull and goat. Rabbit spermatozoa proved to be much less tolerant of glycerol, 2.5% being the maximum concentration which could be introduced rapidly without immobilising the spermatozoa. This concentration gave little protection against freezing. However, when the glycerol was added slowly, as by dialysis, or when it was mixed with methyl cellulose or gum arabic to retard diffusion, effective concentrations were tolerated and good revivals were obtained after slow freezing.

The observations recorded so far all deal with the motility of spermatozoa after freezing and thawing, and it is well known that motility does not necessarily imply capacity to effect fertilisation of the egg. Initial experiments on fertilising capacity carried out with fowl spermatozoa in glycerol-containing media were disappointing, as it was found that the addition of the glycerol without freezing almost abolished fertilising power. Even so, however, results with frozen spermatozoa were not entirely negative. Later on, reasons appeared for thinking that the adverse effect of glycerol was due to its too-rapid removal from the spermatozoa in the oviduct, and it has now been found that if the glycerol is removed slowly by dialysis before insemination, unfrozen spermatozoa retain normal fertilising power, while spermatozoa frozen to −79° C. for a few hours are almost normal in this respect. My colleague Mr. Polge has obtained large numbers of fertile eggs and normal chicks following insemination of spermatozoa so frozen. Freezing for a week or longer, by present methods, is more destructive of fertilising power, but chicks have been produced from spermatozoa frozen for 33 days. Such chicks appear perfectly normal and develop normally. There are some indications that storage in liquid air has certain advantages, and fertile eggs have been produced from spermatozoa so frozen for 12 weeks.

In mammals, results are not yet so encouraging. The use of frozen and thawed bull semen for insemination in a small series of experiments by Mr. Stewart at Reading led to one calf being produced, but in a much larger series of experiments carried out by Mr. Rowson in Cambridge by a different technique, the results were all negative. In rabbits, only one fertilised egg has so far been found among 53 recovered from does inseminated with frozen spermatozoa.

We have, however, already demonstrated beyond doubt that exposure to very low temperatures is compatible with the spermatozoa retaining fertilising power and normal genetic properties, and the technical problems associated with the application of this discovery to the prolonged storage of spermatozoa will undoubtedly be solved. It is thus likely that in the comparatively near future it will be possible to store spermatozoa in a potentially functional state for indefinite periods. We have, therefore, to contemplate the possibility of an animal begetting progeny long after its death; that is the possibility of the addition of a quite fantastic weapon to the armoury of the stockbreeder. We have also to realise that a similar possibility will exist for man. This idea does not violate the biological concept that the soma is merely a temporary and often inadequate vehicle for the essential germplasm, but it is one which will disturb deeply many who regard themselves as more than mere germplasm containers. This work on the preservation of spermatozoa thus provides another example of research of great scientific interest, of which the practical applications may be embarrassing as well as beneficial.

Viscount Samuel, speaking on "The Problem of Ether," said that for good reason physicists have abandoned the idea of an ether, such as nineteenth-century scientists believed in—a kind of rarefied gas or an elastic solid. Yet there must be some medium to carry radiations. Einstein proposed a space-time continuum, but that is merely an invention of the human mind; space and time have no objective reality. It is the same with the whole apparatus of mathematics. These are not dynamic; they cannot, of themselves, *do* anything in the real world. They cannot convey broadcasting waves from Paris to London; or light and heat from the sun to the earth; or let the moon lift the tides of the ocean; or keep the stars and the planets circling eternally. Present-day mathematical physics gives us no answer to the problems, not only of the medium for conveying radiation, but also of the nature of gravitation, and of momentum, and whence come the particles which physicists accept can be newly created.

To find an answer to all these problems together, it is suggested that we must look for some element in the universe which is familiar, constantly employed, and the reality of which is undoubted. When we examine the phenomena of light, heat, motion, weight, electricity, and radio, we always come at the end of the analysis to energy. The proposition is offered that an ether exists consisting of energy, that this is universal, and that it is the sole constituent of the material universe. But it cannot be undifferentiated active energy; that would be "a raging chaos;" it could not be the basis for an orderly cosmos. Nature, however, gives us many examples of entities that exist in two or more states. Water may be solid, liquid, or gaseous; an electric wire is in one state when current is passing and in another when it is not. The essence of the new proposition is that energy also may exist in two states—active and quiescent, passing easily from one to another. The view that there exists an ether consisting of energy was held by J. J. Thomson, the discoverer of the electron. The idea that energy may be in two states, active and inert, is supported by citing a recent statement by Einstein.

All physical phenomena would be conceived as originating in transitions from quiescent energy to active and from active to quiescent. The conception of quiescent energy is not put forward as a logical assumption invented to account for phe-

nomena otherwise inexplicable, but as presenting a real factor in the universe, its existence being a necessary inference from the observed phenomena, the reality of which is not contested.

"If we try to make Science our God, the world will end by calling it the devil," said C. A. Coulson, speaking on "The Place of Science in the Christian Faith."

The apparent separation of science and religion is a relatively recent development, out of line with the earlier development of science. It arose from fear on the part of Christians to face up to new knowledge, and from conceit and arrogance on the part of scientists who claimed too much. Yet it seems today as if we are moving out of those shadows, to a position where both disciplines are seen to have a validity, though restricted, of their own, since they are different partial views of a greater whole. There is common ground in which we find certain central ideas in both, though expressed in different, separately appropriate, terms. Thus both are necessary, as comrades in a common search, fighting, as Max Planck put it, "in a common crusade." The study of science is a religious activity; it is those who deny this, and not the Christians, who are the truly narrow-minded among us. Our science will be seen in its fullness only when it is appreciated from beyond science: our religion will become fully alive only when the splendour of the world revealed by science is bodied forth in awe and in worship and understanding.

A. V. Hill, the distinguished biophysicist who in 1922 was awarded a Nobel prize for his work in physiology and medicine, succeeds the Duke of Edinburgh as president of the British Association. A. M. Tyndall, W. Wardlaw, J. E. Richey, A. D. Peacock, R. O. Buchanan, T. D. Jack, Ben Lockspeiser, E. O. James, R. C. Garry, P. E. Vernon, Meirion Thomas, A. L. Binns, and N. C. Wright were elected presidents of the various sections. Their appointments will extend through the 1952 annual meeting in Belfast, September 3–10.

SMO ON THE AIR

Station	Sponsor	Time
	Sunday	
KLBM, La Grande, Ore.	Baker-Union Department of Health (Research Report)	10:15 A.M.
	Monday	
WOI-FM, Ames, Iowa	Iowa State College of Agriculture and Mechanic Arts (Articles of Interest)	7:45 P.M.
	Tuesday	
WEVD, New York City	Wendell W. Rázim (Science for the People)	9:00 P.M.
	Wednesday	
CKPC, Brantford, Ont.	The Telephone City Braodcast Limited (Modern Science)	9:45 P.M.
	(Irregular)	
KBER, Baker, Ore.	Baker-Union Department of Health (Research Report)	. . . .

The Editor of The Scientific Monthly will be glad to cooperate with university or other educational stations interested in securing scientific material suitable for broadcasting.

SCIENCE ON THE MARCH

BIRD BANDING AND ITS EDUCATIONAL VALUES

THE idea of marking a bird to see if it would return the next year is one of those ideas that arise spontaneously and independently in many places. It is a natural result of the general interest that attends the annual phenomenon of migration in temperate climates. Rydzewski[1] has recently given a resurvey of the early sporadic ventures in the field. We *might* credit (Rydzewski does not) the report of a German monk of the thirteenth century attaching an inscription to a swallow, "Oh, swallow, where do you live in winter?" and receiving the reply, "In Asia, in home of Petrus." The early nineteenth-century report of a stork bringing a reply from India one year and from Japan the following year savors too much of the practical joker. There were no doubt numerous early trials in Europe which never bore fruit or became recorded. Sporadic "unofficial" cases continue to occur.

The first American record by Audubon was probably original with him. His veracity has not been questioned, but his statement of finding two of the (five?) marked nestlings the following year is a high percentage judged by results of modern work. Low[2] secured 12 per cent on tree swallows in one year. During the next one hundred years there were more sporadic cases, and at the close of that period modern cooperative study began in earnest. When taken over by the U. S. Biological Survey (now the Fish and Wildlife Service) in 1920 it was placed upon a really extended program, which resulted in numbered bands being placed upon 2845 birds the first year. Since that date some 300,000 birds have been banded each year, and the ramifications of resulting studies have been numerous.

One of the major interests of the federal service was the waterfowl population. This was admirably susceptible to such inquiry because the birds congregate and are readily trapped; further because the large numbers killed by hunters supply a great volume of return records. These records soon showed that approximately 12 per cent of the ducks banded were killed each year, and led to the conclusion that the number banded must be about the same proportion of the total number of ducks.[3] A product of inestimable value was an increased interest by the people who live in the nesting grounds and related wintering grounds.

Major pathways of migration had been recognized, and now the records from banded birds showed that individuals usually adhered to their regular channels, but that they occasionally joined birds of a different group with which they may have been associated during winter. This was demonstrated by McIlhenny[4] and others in shipment of birds to be released at faraway points. A feature here that still has received little attention is the probability that unusual records of various species may often be due to intermingling with other individuals or species during winter or migration.

Probably the first and most often asked question is, How long does a bird live? This is also one of the easiest to answer, although the calculations have been greatly complicated by heterogeneity of data. Farner[5] computed results from American robins and secured an average of 1.7 years. He cited similar studies on other species, which are in general agreement. We can say confidently that the *average* length of life for ordinary small birds is about two years; that individuals frequently attain five years and, rarely, ten years. An occasional duck has had the phenomenal good fortune of escaping his additional risks to reach twelve to fifteen years. M. T. Cooke[6] listed many long records .

Does a bird return to the same place to nest? This can be answered definitely in the affirmative. Many people were sure this was so, for they had seen a one-legged or otherwise marked bird in successive years. Now we have thousands of cases, many of the birds returning for several years. Evidence that they may go elsewhere is not so satisfactory, for we have no way of accounting for every marked individual. The best results are secured by starting with an adult, nesting bird. This individual has a strong attachment to the location and returns to it, although we still do not know how he is able to do so.

Studies on the common tern at the Austin laboratory[7] over twenty years, and involving 250,000 banded birds, showed that although the colonies changed to some extent the Cape Cod group remained quite constant as a unit. Griffin[8] experimented by transporting gulls and terns distances up to 872 miles and obtained 93 per cent returns from herring gulls. Lincoln[9] listed many return records of various species. The problem of young birds is complicated. Usually the family or the individuals go vacationing after the young leave the

nest. The young can have little attachment to it, and they have less chance than the adults of surviving through the year. With a new year the old birds return to their old place, and the young must find a new one. There is much scattered evidence that young birds are inclined to wander out of bounds. Laskey[10] found a good number of female bluebirds banded as nestlings returning to the same area to nest.

A surprising fact is that birds often return to the same wintering ground. This was noted first with tree sparrows in New England.[11, 12] Bryens[13, 14] secured winter returns from the snow bunting, a species that would seem less likely to behave thus. But the chickadees at my feeding shelf do *not* come back, except for an occasional one that seems to have nested near by. Wallace[15] concluded that their behavior was complicated. Middleton[16, 17] had 77 out of 603 tree sparrows return to winter quarters, some of them for several years in succession, but had much less success with juncoes.

Cowbirds, which build no nest but deposit eggs in the nests of other birds, have a strong attachment to their breeding places. The persistence with which they re-entered traps caused operators to carry them several miles away, but they always came back. Lyon[18] even shipped birds from northern Illinois back to Louisiana and they returned. One bird returned from New Orleans, from Denver, and from Quebec.

How long does it take a bird to fly 500 or 1000 miles? We had good circumstantial evidence and we hoped to get more definite data but here we have largely failed. A bird has *no* attachment to his travel hostelries. Lyon banded 20,000 white-throated sparrows during migration. I have banded 6000 Harris's sparrows. These and others provide no migration returns to the station or records en route. The chimney swift stands apart, for this species is concentrated into a few large chimneys during its stopovers.[19]

Do birds remain mated? The answer is, maybe yes, maybe no. The constancy of swans, geese, and eagles is classical. Perhaps we can condone the temperamental house wren,[20] whose nesting habits have made it subject to minute scrutiny. It should be stated that the long life and small population of large birds favor constancy. The loss of mates, so common in small birds, requires replacements, probably without material change in location.

The above are a few of the questions that come most readily to mind. Resident birds have proved the most useful subjects. Migrating birds might be written off as a loss, but banding does provide an index to numbers and movement. This has been treated with some scientific skepticism on the grounds that it does not equal an observational census and that birds may be held over by abundant feed. On the last point the general evidence is that no more than a few individuals are affected, and there is equal suspicion that other factors are involved with these birds. On the first point, certainly, better results are had with some species than with others. I found[21] that with Harris's sparrow the average stopover in fall migration was about a week and that adults traveled ahead of the young. The latter point could be observed without banding but could not be so accurately defined.

A definite permanent marking of the individual has made possible many detailed studies in behavior. Nice[22] studied family and territory habits of song sparrows on a limited area. Hann[23] studied the ovenbird. Weights of live birds[24] have also been studied. The serially numbered leg bands are the one positive means of identification (subject, unfortunately, to errors in recording), but in most cases the bird cannot be captured at will.

Supplementary markings for sight identification have been used with good results. Butts[25] used colored bands in studying movements of chickadees and nuthatches. His trials with feather painting were not very successful, but advances in plastic coatings have now made the method usable, especially in waterfowl studies. A still more recent method is the injection of dye into the egg to stain the down of the chick. This is being used in upland game studies for temporary identification.

It will be apparent that color banding has limitations. If my neighbor and I both wish to use it we must agree upon a system. Coloring the regular aluminum bands has not been widely used; celluloid bands are less permanent, but a series of colors can be used. The Micheners[26] used these in studying mocking birds in their locality. Extended plans for their use on colony nesting birds have been carried out in several large areas.[27, 28] In these all birds from a particular colony or group of colonies were given the same mark, so that only place of origin could be determined by a sight record. Mason[29] has recently proposed a general system of colored bands for the Bohemian waxwing, a flocking species whose eastward winter movement has been of special interest.

Banding has made possible the accumulation of a large amount of information. Some of it has been of immediate practical value; much of it is of essential scientific value. In my opinion the intangible values that may be grouped under

"education" are still more important. Bird study has always been a popular avocation. Those who take part in trapping and banding constitute so far a mere handful—less than 5000—but those who have an interest in bird life are reckoned in the millions. Thus the banders have an important place as teachers.

One of the foremost exponents of bird study, the late Frank M. Chapman, took special pleasure in transposing the old saying "A bird in the hand is worth two in the bush." Reversing it, he indicated that a living bird that could be studied was worth more than a dead specimen. With the advent of trapping and banding we may well return to the original form. The bird now in the hand is a living, normal specimen, restrained for only a few seconds or minutes. It is not far up in a tree where we cannot see it clearly; it is before us where we can examine each detail, the partly concealed markings as well as the more evident ones. Best of all, we can examine a large series, perhaps hundreds, without destroying or injuring a single one. We can observe plumage changes in those frequently caught.

We can observe behavior to some extent. Reactions to capture vary with species and with individuals. Rarely do any birds become tame enough to allow capture at will, but they often acquire a comparative indifference to the process. We can perform some service by removal of parasites and by minor surgery.

Banding has been used by many teachers as a part of biology courses or as supplementary material. It has great possibilities, but most schoolyards are not suitable as trapping stations. My own station has been visited by many school and Camp Fire groups, not to mention numerous individuals. Among the latter was Norman Criddle, a well-known ornithologist of Manitoba. He had been banding a few birds as they came to land, but shortly after his visit he wrote that he had begun trapping. The visitor always gets a thrill out of the opportunity to observe the living birds and to learn something of their behavior. For myself, I feel that one interested visitor compensates for hours of effort that at times had seemed not worth while.

Banding, like other phases of bird study, can be done on a large or a small scale. The Austin laboratory on Cape Cod counts its annual total in tens of thousands. Large numbers are usually the result of the flocking habit, but many moderately large records are accumulated by constant effort over a period of years. Large numbers are desirable or necessary for certain interpretations.

Some operators travel widely and some endure physical hardship. Two of my friends waited in a car in the coldest weather, and another nearly suffered frost-bitten feet, because only such weather brought snow buntings. Broley,[30] after having retired from "active" life, climbs trees to eagle's nests. Coffey penetrates the swamps for heron nests. Many more operate only in their backyards, but each of these can make important contributions by careful study of common species. All can make large, even if intangible, contributions to appreciation of bird life.

The bird bander may begin with little thought beyond banding as many birds as possible. Certain it is that it will not be long before he begins to ask some of the many questions that are naturally suggested by various features of his work. His education will progress far more than that of his visitors. He is almost certain to become an investigator to some degree. Here is where he should have assistance from those who can suggest projects toward which he can contribute and can help him make his work effective. If we are to divide bird students into "ornithologists," "bird banders," and the like, we shall make the best progress if we can have mutual assistance among the groups.

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SOIL CONSERVATION PRACTICES IN THE CARIBBEAN ARCHIPELAGO

DURING a recent tour through the Caribbean Archipelago, I observed soil conservation practices on St. Thomas, St. Vincent, and Montserrat that seem worth reporting.

Very little farming is carried on in St. Thomas. What little there is, is done by the farmers of French descent, and is mostly limited to the northern coast. It was around these French rural settlements that the Agricultural Experiment Station, under the USDA, was established a number of years ago. The station endeavored, among other things, to establish soil conservation practices that would not only permit the efficient utilization of the land, which is very scarce indeed, but also offer the farmers an example worth following. Figures 1, 2, and 3 show the method and the results. The method seemed so attractive that the neighbors took to it readily and soon put it into practice on their own plots. The hard labor is done during the late afternoon hours. As can be observed from the photographs, this section of the island is very steep and rocky, and it is normally

FIG. 1. A level terrace built on a 35-degree slope, northern St. Thomas.

FIG. 2. Rocks are used to hold the land and form the "bench" for the terrace.

FIG. 3. Lettuce, shallots, radishes, and other vegetables thrive well. (Figs. 1–3 were taken at the USDA Experiment Station.)

FIG. 4. Another successful terrace on St. Thomas.

covered with xerophilous scrub. The rainfall amounts to about 45 inches a year, but is poorly distributed. Before terraces came into use, the nomadic practice of clearing a spot, exploiting it for two or three seasons, and then abandoning it was in vogue. Now, although a great deal of work is required at the start, the plot is not abandoned. While a first crop of bananas and/or dasheens is being raised the terraces are being built, slowly but surely. Usually afterwork hours are used, to avoid the heat of the day, and neighboring farmers may help each other. In the course of this labor some land may be washed out if a shower hits while the soil is loose and bare, but once past this stage the land is very effectively conserved (Fig. 4). Some of the farmers have made enough progress to afford a truck and a garage. Rainwater from the roofs of the houses is caught in tanks or in small catchments (Fig. 5), and, as the farmer prospers, it may be piped to the garden and the plants sprayed by hose. Thus the French farmers of northern St. Thomas can provide the local market nearly the year round with at least part of the fresh vegetables needed.

The situation on St. Vincent is entirely different, for this island is largely agricultural. The soils are of volcanic origin, and on the whole are porous and well drained. Rainfall is much higher, but frequent drains are obviously unnecessary. The island produces 70 per cent of the world's arrowroot starch and a large proportion of the peanuts raised in the West Indies. These are raised on both large estates and small holdings, but from our observations, everyone seems to be soil conservation-minded. Vetiver, or khuskhus grass (*Vetiveria*

FIG. 5. Water from roofs and water-catchments is led to tanks and then piped into the gardens.

FIG. 6. In St. Vincent, British West Indies, khuskhus (*Vetiveria zizanioides*) is planted in rows on contour lines to hold the soil.

zizanioides), is widely used to hold the soil in place (Fig. 6). Frequent cutting keeps the grass from going to seed, and the tops are used for mulching. Occasionally strip planting is practiced, alternating arrowroot and other root crops like yams, taniers, etc. Agricultural officers are not yet satisfied, but do not like to recall the days when the people would not even let the government lay the contour lines and plant the grass for them, free of charge.

Montserrat, one of the Leeward Islands, also practices soil conservation. In places where rocks are abundant, they are placed in heaps, or occasionally in rows, heaps starting on top of another large rock to save space. Even in apparently level places, where erosion seems negligible, rows of khuskhus can be seen planted to conserve the soil.

Suffice it to say that the little islands of St. Thomas, St. Vincent, and Montserrat have soil conservation practices worth observing by some of the islands where erosion is still an unsolved problem, and where heavily eroded slopes are frequently seen.

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BOOK REVIEWS

SCIENCE, SELF-TAUGHT

Fundamentals of Atomic Physics. Saul Dushman. x + 294 pp. Illus. $5.50. McGraw-Hill, New York. 1951.

As a species, the autodidact may never have been extensive, but certainly nowadays in the midst of broad educational facilities he does not seem to be common. Yet, in a sense, this volume by Dr. Dushman is specifically dedicated to those for whom recent popularizations of science are unsatisfactory and whose preparation and interest preclude the use of the official authoritative classroom texts. Originally the material was prepared for a concentrated course given to high school teachers to acquaint them with the new developments in the physical sciences. It was then broadened to encompass the interest of students of engineering in matters atomic. To the former, the inclusion of a review chapter on the requisite mathematics should prove useful. For the latter, the extensive use of graphs and tables and the frequent comparison of atomic magnitudes with the more familiar macroscopic values encountered in chemical reactions are appealing features.

The introductory chapter on the history of physics is necessarily sketchy, but sufficiently clear lines of demarcation are drawn between the different stages of development to convey to the reader the basic changes wrought in the mode of thought characteristic for each epoch. The few errors and the brief mention of certain contributors are offset by the coherence of the chapter as a whole and the judicious selection of material pertinent to the appreciation of the physicist's approach to problems. The mathematical aids are covered in Chapter 2, and the text material proper begins with the kinetic theory of gases. The treatment of this subject includes a discussion of the Maxwell-Boltzmann distribution law, mean free path, transport phenomena, and molecular size, with a number of illustrative applications. A simple derivation of the expression for the rate of incidence of molecules on a surface is not included. Its omission will be confusing to some readers, and its inclusion would have been helpful in the discussion of rates of effusion and the transport effects. Evaluation of the work of Thomson and Millikan on the electron is extended to include the revised values prompted by measurements on X-ray wavelength, the relativity considerations on the mass variations with speed, and the mass-energy relation. The chapters on thermionics and on photoelectricity are brief but contain several good tables and graphs. However, the work function is appropriately discussed, and in the following section on X-rays we again find only a brief treatment of the general phases, highlighted by a stress on the Compton effect and its significance for the photon theory. The experiments on the scattering of alpha particles leading to the nuclear model, and the Bohr treatment of the hydrogen model, serve to introduce the discussion on optical and X-ray spectra. The atom model is not developed sufficiently, however, to clarify the basis for the Pauli exclusion principle. Its application to the periodic arrangement of the elements is extended to a discussion of the transuranic series and to their chemical properties. The single chapter on matter waves is ambitious, and, considering the type of student for whom the book is intended, it is a laudable effort. The basic ideas of the Schrödinger equation are developed, and the reader is given some insight into the broad scope of its use and results.

About one third of the book deals with nuclear problems. Here every author is challenged to make a selection appropriate to his goal. Dushman has again chosen to deal with some of the highlights. These include the discovery and properties of the fundamental particles, binding energies, the quantum-mechanical interpretation of the Geiger-Nutall rule, and illustrative examples and tables of the various types of nuclear reactions. The views of stellar energy and the utilization of fission are of course included. For engineering students a more detailed description of nuclear piles and installations might have been expected, but the author here sticks close to basic processes. On the other hand, the last chapter, on particle accelerators, is more complete in this respect. The elementary theory underlying magnetic resonance acceleration is developed and applied to all the well-known types of machines in use, as well as to the microtron. The exposition is sufficiently detailed to enter into a discussion of the phase requirements, variation of mass with speed, focusing action, etc.

A commendable feature of the book is the extensive references to more authoritative sources, though few of these are to original papers. The student is made to recognize in each section that the treatment in the text is of only a general exploratory nature at the foundation and ground level. This stimulus to further reading is here done much more consciously and extensively than in comparable texts. The choice of topics for a course designed to acquaint high school teachers of science with modern developments is good. Whether the same choice will appeal to students of engineering is difficult to judge. Very little of the material can be considered superfluous, and to amplification there is no limit.

The book is adequate to introduce those trained in general and applied science to some phases of atomic physics. It is not described as a text, but with the addition of some provocative questions and problems, and in the hands of a good instructor, it could well serve as the basis of a course that does not bear the title "Survey."

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BUFFALO HISTORY

The North American Buffalo. Frank Gilbert Roe. vii + 957 pp. Illus. $12.00. University of Toronto Press, Toronto. 1951.

A TREATISE of exceptional merit has been added to the historical literature on the American buffalo, by Frank Gilbert Roe, a scholar of history and science. In this critical review of past research and investigation of early writings and documents the author has given us a faithful and accurate report of data combed from a prodigious number of original sources. Quotations and footnotes are used extensively in an attempt to present facts from which the reader may formulate his own opinions.

Some ideas that have become dogma concerning buffalo characteristics are thoroughly and, it seems, decisively discredited by the author, who cites in his own support dissonant observations recorded by equally reliable individuals during approximately contemporaneous periods in essentially the same regions. He takes strong issue with those who have been careless or prejudiced in their reporting or research but readily supports those who have based conclusions on accounts of original observations, realizing, of course, that the human factor requires caution even there. Catlin, whose observations were condemned on all subjects because of his sympathy for the Indians, is vindicated.

The long-accepted myth of the seasonal migrations between Texas and Canada is proved beyond question to be unsubstantiated. Local, somewhat regular movements of restricted extent seem verified, but most firsthand observations suggest random wandering rather than repeated seasonal migrations between specific areas. Other ideas such as the buffalo trail origin of our roads, the superior intelligence attributed by some to the buffalo, the existence of several variants in historic times, the recent geographic range, all receive detailed examination in the light of historic records, with the result that drastic revisions and often opposite conclusions from those prevalent today are reached. Considerable discussion is devoted to agencies other than man, including animal foes, fire, water, and snow, which were destructive to buffalo. The records are searched for an estimate of the number of buffaloes present at any one time since their first discovery by Europeans, but this remains a difficult question to answer with certainty. The accounts of their final extermination in the wild state at the hand of man is a shameful episode in American history almost equaling the disgraceful exploitation of the Indian, whose defense of himself and his land has normally been misrepresented as inherent savagery. Finally, the influence of the buffalo on Indian mentality is considered, with suggestions which have certain implications regarding the nomadic tendencies of the Indians at the time of first contact. Utilization of the buffalo appears to have merely facilitated a pattern of existence already the mode for most tribes.

An extensive appendix dealing with many interesting minor facets of buffalo history, plus a large, valuable bibliography, without doubt make this the best documented research yet done on this subject. A healthy skepticism of inadequately substantiated current ideas pervades the work. Although the book is intended primarily as a factual presentation, certain conclusions could hardly be avoided, and where these are not in accord with accepted beliefs one must concede that Mr. Roe has strong supporting evidence for his contentions.

This book can be recommended without reservation to anyone interested in an accurate concept of the historic American buffalo based on an impartial survey of observations recorded by those who had firsthand knowledge of them. It should, in addition, prove profitable reading for all who have an interest in the early explorations of North America and in the Indians from the time of Coronado until the late nineteenth century. More than that, it is a model for any scientific investigation requiring use and interpretation of early writings and records.

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EXTENDING FREUDIAN DOCTRINE

Mental Health and the Prevention of Neurosis. Joachim Flescher. 608 pp. $5.95. Liveright, New York. 1951.

DURING the past few years several psychoanalysts have published texts that were intended to clarify or extend Freudian theory. Dr. Flescher's book is in this tradition. Written for the educated layman, it is a highly technical book that deserves close reading.

Dr. Flescher presents, in the main, a fairly orthodox picture of psychoanalysis, but he has extended Freudian doctrine in at least two important ways. The aggressive drives are given much more attention. Also, the practical application of psychoanalytic principles to the *prevention* of neurosis is stressed.

Aggression and Eros are seen as the two primary sources of dynamic impluses, Eros working on behalf of growth, development, and union; aggression toward decay and annihilation. Under normal conditions, aggression and Eros are in equilibrium in the individual, and most behavior represents a fusion of the two instincts. Under certain abnormal conditions, which Dr. Flescher spells out, the equilibrium is distorted and a diffusion or separation of the instincts takes place. Flescher postulates that excessive inhibition of the erotic drives exercises a stimulating influence on the aggressive tendencies. Inhibition of the aggressive tendencies, however, does not intensify the erotic tendencies. On the contrary, the release of aggression can result in the strengthening of the Eros.

Conventional education has been in error in tolerating or encouraging only the sublimated expression of the erotic drive, and not tolerating aggression at all. Mere inhibition of aggression, without adequate education for its control, can only result in unhealthy psychic and physical expressions. Psychosomatic illnesses, physical illnesses, even premature aging, can result from improper handling of aggressive impulses. Disturbances of

the erotic drives also occur when aggressive impulses are improperly curbed. For the promotion of psychic health, then, Flescher recommends that conventional education be replaced by psychoanalytically oriented education.

Differences between male and female psychosexual development, both normal and abnormal, are interestingly and provocatively discussed. In this treatment, somewhat more recognition is given to cultural factors than is common in orthodox psychoanalysis. "Culture" is limited largely, however, to middle class mores.

Case material is interestingly, though sparingly, used for illustrative purposes.

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EARLY MAN AND HIS WORKS

The Civilizations of Ancient America. Sol Tax, Ed. viii + 328 pp. Illus. $7.50. University of Chicago Press, Chicago, Ill. 1951.

THERE has been general agreement among archaeologists for many years that the civilizations of Middle America and the Central Andes had a common cultural basis. This hypothesis now seems to have reached the "generally accepted theory" stage, although the evidence on which it is based still seems slim indeed. In editing the essays and reports that constitute *The Civilizations of Ancient America,* Sol Tax has brought together for the first time in comprehensive form a considerable mass of material bearing on common problems of both regions.

The book is one of three volumes of selections taken from papers given before the Twenty-ninth International Congress of Americanists, held in New York September 5–12, 1949. Two other volumes are entitled *Indian Tribes of Aboriginal America,* and *Acculturation in the Americas.*

The introduction is by Wendell C. Bennett, professor of anthropology at Yale University. Dr. Bennett's contribution in discussing "Nuclear America" was that of providing a general background for the collected papers, whose scope of coverage is enormous. He shows that the advanced culture in the Western Hemisphere in pre-Columbian times was not a sudden development but extended back through many centuries; that although many scholars have worked in the two major subdivisions of the area there is a need, too, for considering nuclear America as a whole.

The proceedings of the congress are recognized everywhere as valuable source material, but in this printed form it is expected that the papers will be more widely read. The names Aztec, Maya, and Inca are familiar to all literate people today as the local examples of cultures through which the Indians of Middle America passed at a time when Europe was in its Dark Ages. Even for those with only a slight amount of scientific curiosity about the cultures of Middle and South America the present volume provides a wealth of new information on human life and the activities of those who built the first great cities in the Western world. Their public buildings, relics, monuments, and potsherds turn up as amazing testimony, which needs to be collected periodically in volumes such as these.

Use of Spanish, French, and English in the textual material presented allows for a healthy cross-fertilization of ideas on subjects that in some instances are highly controversial: for example, the significance of certain bound and tied procumbent figurines, novel interpretations of structure in the Pyramid of the Sun at Teotihuacán, new discussions on correlations of the Mayan and Christian calendars, evaluations of the principal forms of visual art as developed in Meso-America and South America, analyses of population density in the Mayan Empires, or attempted parallelisms in the symbolic arts of southern Asia and Middle America.

Nuclear America presents a rich field for both archaeological and anthropological research in all their aspects. In time it covers a relatively long period of development, probably around 5000 years, although most of the finds and much of the best preserved material seem not to be older than 2000 years. There is so much miscellaneous evidence, and such a small portion of the total area has been explored and published, especially in the intermediate subdivision, that many such volumes as this one are needed before anything like a consistent historical framework is possible.

Especially sketchy is the protoarchaic period; hence the special value of papers presented here by Jacinto Jijon y Caamano on civilizations of Middle America and northeastern South America; Gordon R. Willey on the Monagrillo Culture of Panama; Edith Jimenez de Munoz on Colombian symbolism; and Victor Oppenheim on Andean glaciation and pre-Columbian man. These chapters, if they do nothing else, point up the need for a great deal of spadework throughout the intermediate area, from Colombia north to Honduras. Few of the sites studied thus far show evidence of much antiquity; however, there are exceptions discussed in this book, notably the Playa de los Muertos in Honduras, recent finds at Monagrillo in Panama, and perhaps San Agustin in the highlands of Colombia.

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BRIEFLY REVIEWED

Soil and Freshwater Nematodes. T. Goodey. xxvi + 390 pp. Illus. $7.00. Methuen, London; Wiley, New York. 1951.

DR. GOODEY'S book is a reference work designed to facilitate identification of nematodes found in soil, fresh water, plant material, and similar habitats. Since part of their life, at least, is spent in the soil, some material on the nematode parasites of plants and of insects is included. There is a short appendix dealing with the hairworms (Gordiidae).

The introduction contains a section on techniques used in isolating these small worms from the soil or plant

material and a general discussion of nematode structure. The remainder of the book includes descriptions of the higher categories used in classification of nematodes, of each genus, and a description illustrated description of either the type or a representative by line drawings of either the type or a representative species of each genus. In addition, the habitats, distribution, and bionomics of the genus and of the species described are more or less briefly discussed. The other species of the genus are listed, with synonyms.

Since the book brings together for the first time a considerable amount of information from widely scattered sources, it is a distinct convenience, and it is evident that a great amount of labor has gone into its preparation. Certain additions would have increased its usefulness, however. One of these is a key to the genera, and another is definitions of the terms used to denote various structures. References to the best available literature dealing with each genus might also be desirable, since the most useful publication on a genus is often not the one in which it was originally described.

In general, the illustrations are the best available in the literature, but nearly all have lost quality in the process of reproduction, perhaps because of the grade of paper used, but partly because of inferior presswork.

A. L. TAYLOR

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A Century of Science. Herbert Dingle, Ed. ix + 338 pp. Illus. $4.75. Roy Publishers, New York. 1951.

THIS book of nineteen chapters is the work of eighteen authors, including the editor. All are British scientific men with institutional connections, mostly educational. The work is a review of progress since 1851, produced in connection with the 1951 Festival of Britain. The book is well gotten up, with attractive appearance. Each author presents a scholarly review of progress in some field of science, and the editor closes the book with a general review. Bibliographical references are given only incidentally, and there are only a few pictures and diagrams. A detailed index is included.

The chapters are divided as follows: Physics and physical chemistry, 5; general chemistry, 1; biochemistry, 1; geology and the atmosphere, 2; astronomy, 2; phases of biology (evolution, genetics, physiology), 3; anthropology, 2; medicine, 1; psychology, 2. This is also the approximate order of presentation.

In each chapter the treatment is similar. The development of knowledge and theory through the past century, and even before, is treated, with numerous names and dates and with only as much technical material as is required by the discussion of history. The writing is admirably clear and readable.

The viewpoint is definitely British, but advances made in other countries are noted. Physical sciences receive more stress than biological or social sciences, and zoology and botany are not treated as such. The general progress made in the application of science to agriculture, and in biometrics, is mentioned only incidentally. (Agriculture is treated in a companion volume on technology.) The editor expresses the opinion that boundaries between fields of biology and other sciences are disappearing and that discoveries of the past century are revolutionary.

This book is a definite addition to the literature of science and culture.

F. M. WADLEY

Department of the Navy

Fluorine and its Compounds. R. N. Haszeldine and A. G. Sharpe. 153 pp. $1.75. Methuen, London; Wiley, New York. 1951.

THIS is a concise volume that should be indispensable for anyone entering the field of fluorine chemistry in an academic or an industrial laboratory.

The first chapter is devoted to the history of fluorine and a general introduction to its properties and chemistry. A special chapter on HF describes its preparation, properties, and principal uses, with a section on BF_3 as a catalyst.

The third chapter describes the preparation of inorganic fluorides from the first seven groups of the periodic table and the first, second, third, and fourth transition series. The fourth and final chapter occupies more than half the book and deals with the preparation of organic compounds of fluorine.

The text is supplemented by a very adequate list of references, and an index.

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Nitrogen in the Life of Plants. D. N. Prianishnikov. Trans. from the Russian by S. A. Wilde. ii + 109 pp. Illus. Mimeo, paperbound, $2.75. Kramer Business Service. Madison, Wis. 1951.

NITROGEN nutrition of plants was the subject of the Master's thesis prepared by the author in 1895. This subject continued to occupy his attention during the fifty years of his scientific work at the Timiriazev College of Agriculture near Moscow, and was the subject of his last treatise. The translator notes that the book "was originally published by the Academy of Science, USSR, in 1945. It presents an account of the vital problems in nitrogen nutrition, written with a thoroughness characteristic of all of Prianishnikov's papers."

The historical account of the significance of nitrogen in plant nutrition provides the setting for the period during which the author's investigations were started. It is brief but authoritative. An attempt has apparently been made to evaluate all the important information dealing with the relation of nitrogen to plant nutrition; particular consideration is given to the author's own studies, mainly those dealing with the uptake of nitrogen in the form of ammonia and nitrate, factors affecting their absorption, synthesis of amino acids and pro-

teins, and transformations of nitrogen compounds in plants. One is thereby provided with "Prianishnikov's point of view in a form that English-speaking scientists can appreciate."

The scope of the book is indicated by the following titles of the seven chapters: From the History of the Nitrogen Problem; The Forms of Nitrogen Available to Plants; Nitrogen Metabolism in Plants and the Role of Ammonia and Amides; Synthesis of Organic Nitrogenous Compounds from Nitrates and Nitrites; The Assimilation of Free Nitrogen by Plants; Relation of Plants to Ammonia and Nitrate Nutrition Depending upon the Reaction of the Medium, Concentration of Solution, and Supply of Carbohydrates; Significance of Accompanying Cations and Anions in Ammonia and Nitrate Nutrition.

The translator and the collaborators that contributed notes and interpretation of the text are to be commended for their service in making an English version of this treatise available to plant physiologists.

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Ecological Animal Geography. (2nd ed.) Based on *Tiergeographie auf Oekologischer Grundlage,* by the late Richard Hesse. Second edition prepared by W. C. Allee and Karl P. Schmidt. xii + 715 pp. Illus. $9.50. Wiley, New York. 1951.

DESPITE the rearrangement of the title page which places the name of Richard Hesse in second billing, so to speak, this book will continue to be known as "Hesse, Allee, and Schmidt." It has been refurbished, rearranged a bit, and augmented with new material in many places. The bibliographies have not only been brought up to date, but they have also been presented in more extended form to include the titles as well. This alone accounts for a substantial portion of the 118 new pages of this edition. New material has been added to the discussions of barriers, bipolarity, intertidal zonation and limnology, and many other subjects. This has all been worked into the original text rather than appended to various chapters. A few illustrations have been added, but the figures are for the most part the familiar friends of the previous edition. There have been a few oversights—recent work on the Black Sea and estuaries has been overlooked, for example—but the book as a whole remains one of the few indispensable contributions to the subject of biogeography.

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New Books

The Genera of South African Flowering Plants. (Rev. 2nd ed.) Botanical Survey of South Africa, Memoir No. 25. E. Percy Phillips. 923 pp. £2. Government Printer, Pretoria, Union of South Africa. 1951.

The Temporomandibular Joint. Bernard G. Sarnat, Ed. A monograph in "American Lectures in Dentistry," Edward J. Ryan, Ed. (American Lecture Series No. 134.) xviii + 148 pp. Illus. $4.75. Thomas, Springfield, Ill.; Ryerson Press, Toronto; Blackwell Scientific Publications, Oxford, Eng. 1951.

Hormones: A Survey of their Properties and Uses. Council of the Pharmaceutical Society of Great Britain. xii + 220 pp. Illus. 35*s.* Pharmaceutical Press, London. 1951.

The Study of Instinct. N. Tinbergen. xii + 228 pp. Illus. $7.00. Clarendon Press, Oxford, Eng. 1951.

Principles of Radio. (6th ed.) Keith Henney and Glen A. Richardson. vii + 655 pp. $5.50. Wiley, New York; Chapman & Hall, London. 1952.

Amphibians of Western North America. Robert C. Stebbins. 539 pp. Illus. $7.50. University of California Press, Berkeley and Los Angeles. 1951.

Textbook of Organic Chemistry. (3rd ed.) George Holmes Richter. vii + 762 pp. $6.75. Wiley, New York; Chapman & Hall, London. 1952.

Plastics Molding. John Delmonte. vii + 493 pp. Illus. $9.00. Wiley, New York; Chapman & Hall, London. 1952.

High Polymers, Vol. VIII. *Copolymerization.* Turner Alfrey, Jr., John J. Bohrer, and H. Mark. x + 269 pp. $6.80. Interscience, New York and London. 1952.

Substances Naturelles de Synthèse, Vol. III. Leon Velluz, Ed. 156 pp. Illus. 1750 Fr. Masson, Paris. 1951.

Elements of Ceramics. F. H. Norton. xiv + 246 pp. Illus. $6.50. Addison-Wesley, Cambridge, Mass. 1952.

The Enzymes, Chemistry and Mechanism of Action, Vol. 2, Pt. 2. James B. Sumner and Karl Myrbäck, Eds. xi + pp. 791–1440. Illus. $14.00. Academic Press, New York. 1952.

The Chemistry of Synthetic Dyes, Vol. I. K. Venkataraman. xvi + 704 pp. Illus. $14.50. Academic Press, New York. 1952.

The Explanation of Human Behaviour. F. V. Smith. 276 pp. $2.75. Macmillan, New York. 1952.

A Sex Guide to Happy Marriage. Edward F. Griffith. 352 pp. Illus. $3.00. Emerson, New York. 1952.

Range Management, Principles and Practices. Arthur W. Sampson. xiv + 570 pp. Illus. $7.50. Wiley, New York; Chapman & Hall, London, 1952.

The Integration of Behavior, Vol. I. *Basic Postulates.* Thomas M. French. xi + 272 pp. $5.00. University of Chicago Press, Chicago. 1952.

WE DIDN'T MEAN TO DO IT!

NO ONE in our plant will admit making the error in putting together THE SCIENTIFIC MONTHLY for February, so suitable disciplinary action has not yet been taken. Advertising pages v and vi should have run in the front of the book. As it is, we are sorry to say, we have made page v look like part of "Association Affairs," whereas it is, of course, a continuation of "Science and Technology" from page iv.

We offer our sincere apologies to advertisers, subscribers, and editors.

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